42185/B

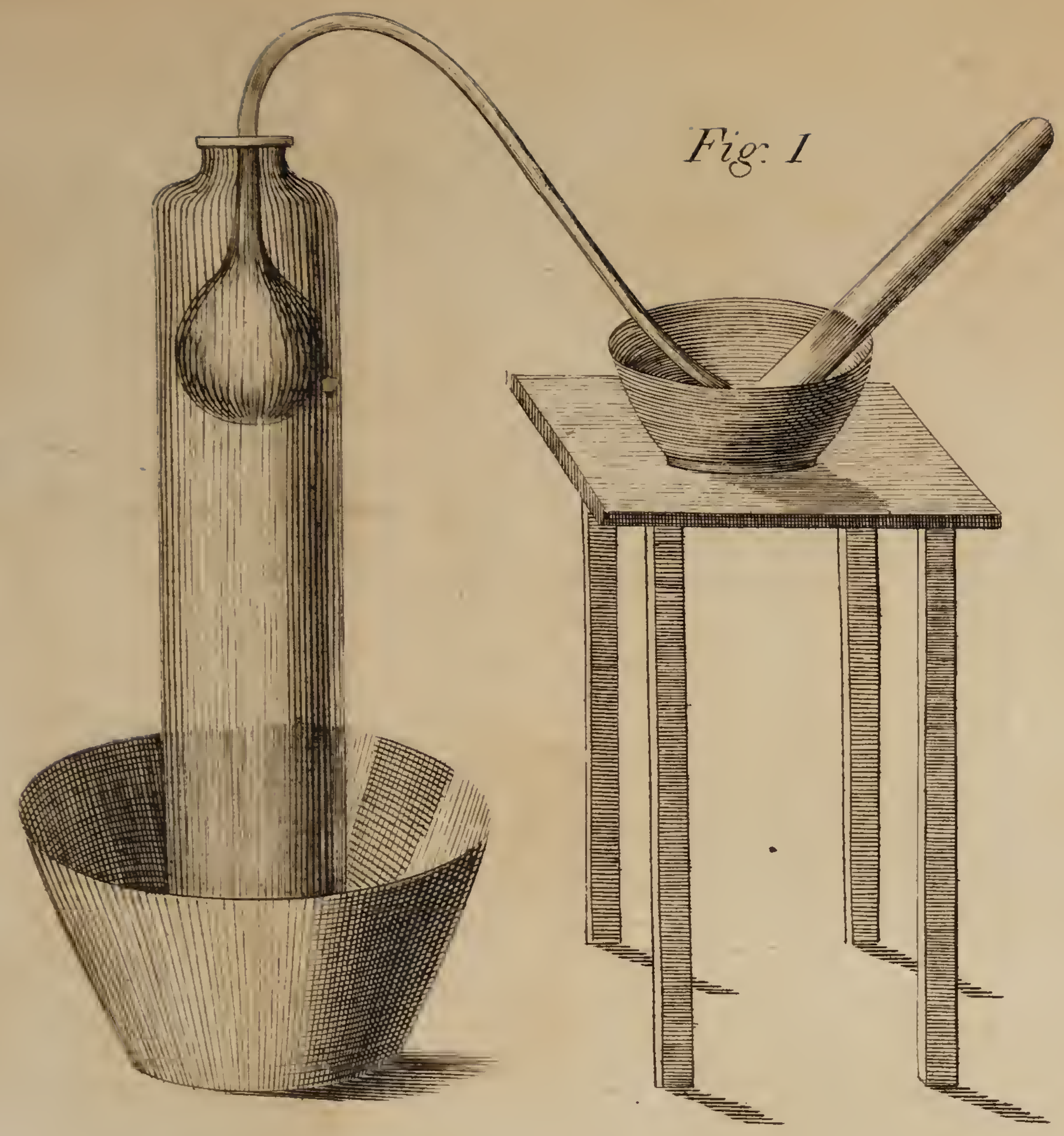
Fig: 1

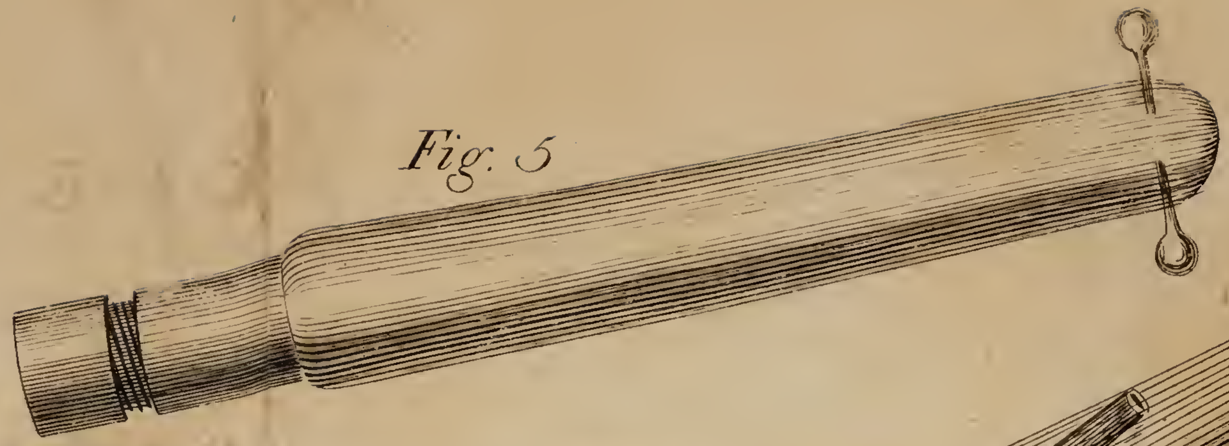
Fig. 5

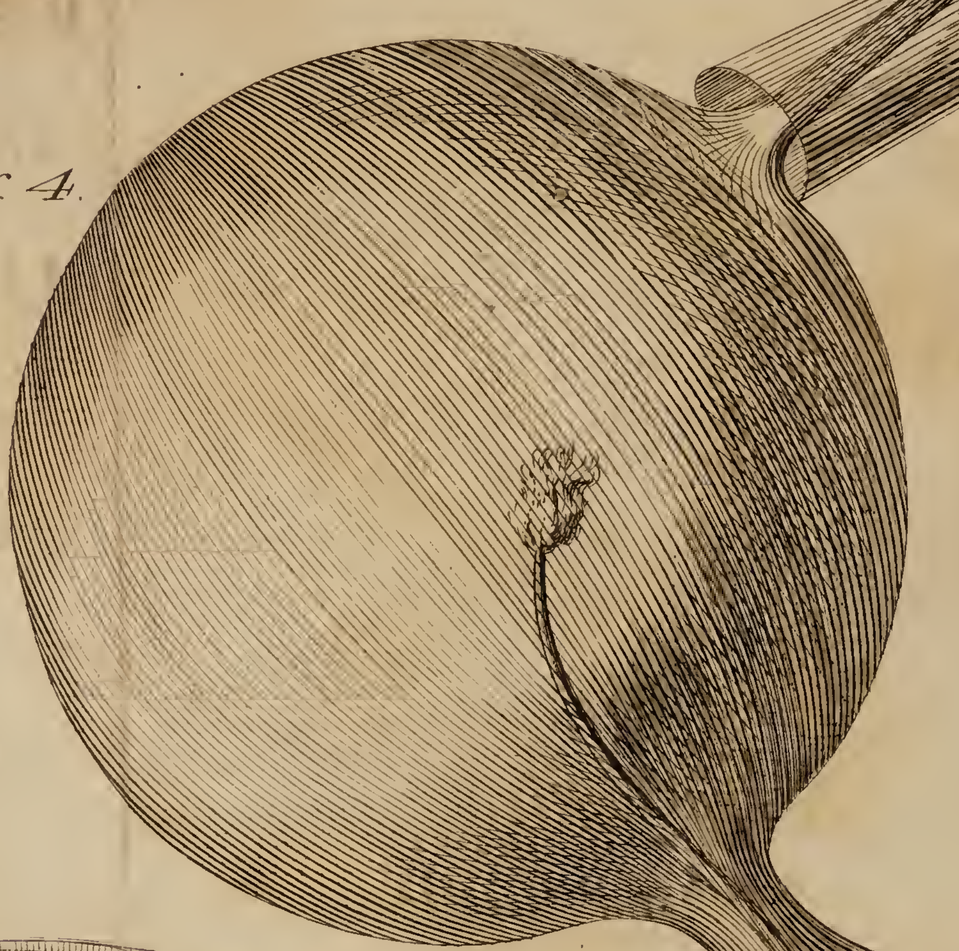
Fig. 4.

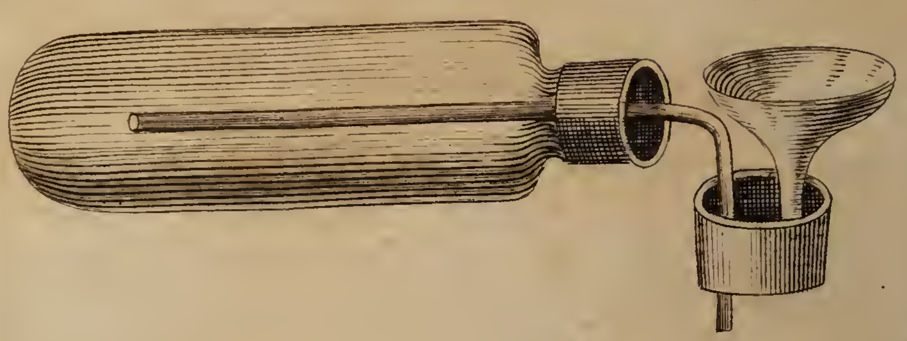
Fig 3

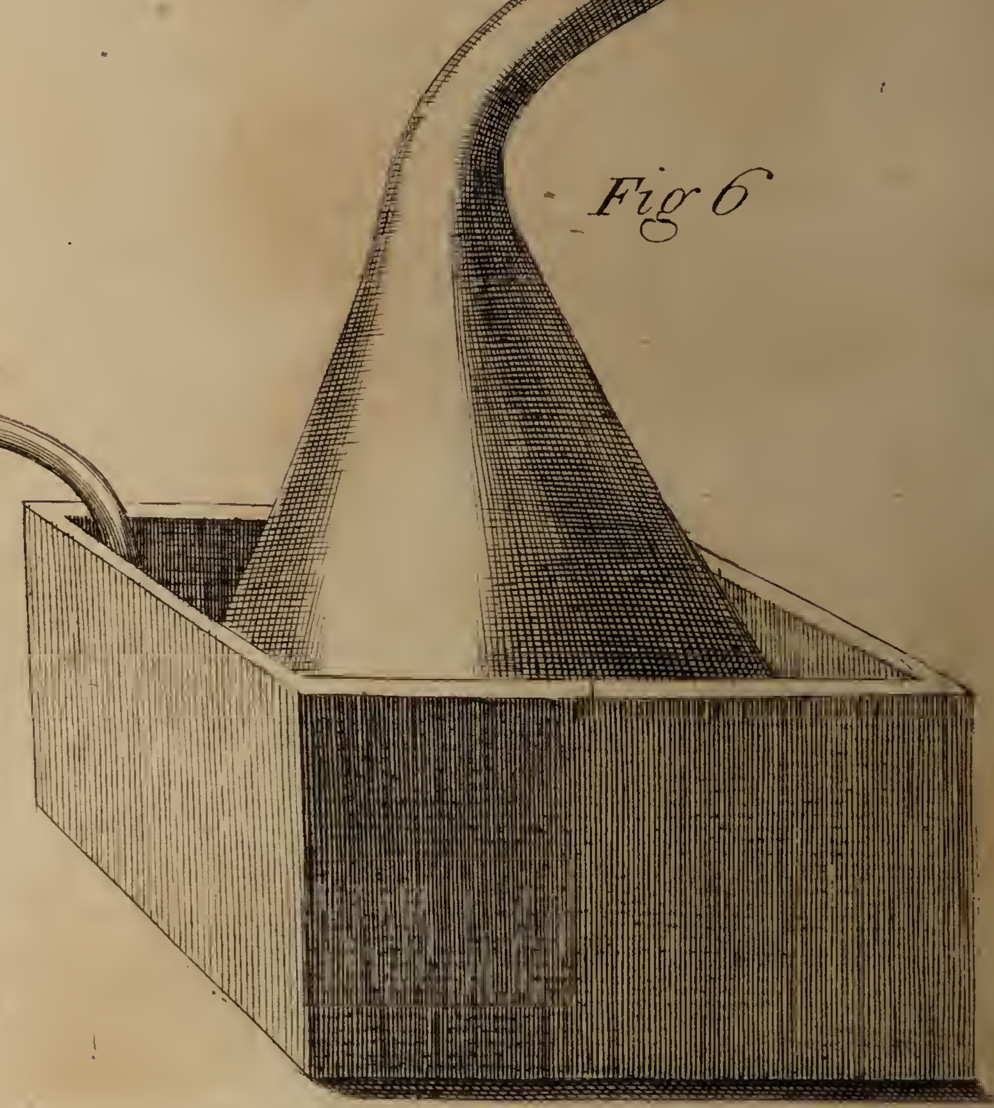
Fig 6

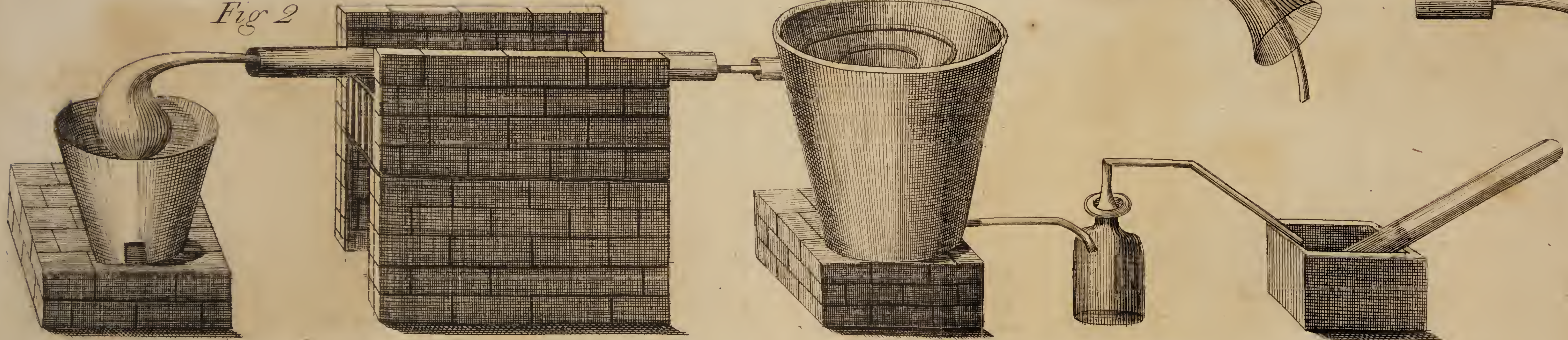
Fig 2

EXPERIMENTS
AND
OBSERVATIONS,
RELATING TO VARIOUS BRANCHES OF
NATURAL PHILOSOPHY;
WITH
A CONTINUATION
OF THE
OBSERVATIONS ON AIR.

VOL. III.

By JOSEPH PRIESTLEY, LL.D. F.R.S.
AC. IMP. PETROP. R. PARIS. HOLM. TAURIN. AUREL. MED.
PARIS. CANTAB. AMERIC. ET PHILAD. SOCIUS.

> ----Trahit quodcunque poteſt, atque addit acervo.
> HORACE.

BIRMINGHAM,
PRINTED BY PEARSON AND ROLLASON, AND SOLD BY
J. JOHNSON, NO. 72, ST. PAUL'S CHURCH-YARD, LONDON.
MDCCLXXXVI.

300418

WELLCOME
HISTORICAL
MEDICAL
LIBRARY

TO

WILLIAM CONSTABLE, Eſq;

OF

BURTON CONSTABLE, IN YORKSHIRE,

THIS VOLUME

IS,

WITH THE GREATEST RESPECT
AND GRATITUDE,

INSCRIBED,

BY HIS OBLIGED,

HUMBLE SERVANT,

J. PRIESTLEY.

THE

PREFACE.

THIS volume, like each of the five preceding ones, contains an account of many *new facts*, with ſeveral illuſtrations of thoſe diſcovered before; but withal many *new difficulties*, ſuch as muſt excite a deſire of making farther progreſs in theſe inveſtigations: ſo far are we from ſeeing any end of our labours.

This, however, is a conſideration by no means diſcouraging to the true philoſopher. On the contrary, it is calculated to open to him the moſt pleaſing of all proſpects, as it ſhows us that there will never be wanting proper objects of theſe inveſtigations, than which nothing can furniſh more delightful employment to the mind of man, and which promiſe advantages that will abundantly compenſate for all the labour that we can beſtow upon them.

As we ourſelves are parts of the great ſyſtem of nature; as the laws of nature comprehend, and continually affect us; and every thing that we do is putting things into ſituations, in which the laws of nature determine the reſult; the more perfect knowledge we have of thoſe laws, the better we muſt be able to foretel thoſe reſults, and therefore to chuſe what we wiſh to produce. Conſequently, the more knowledge we have of the laws of nature, the greater power we ſhall have over nature, and the more commodious we ſhall be able to make our ſituation in the world, while it remains conſtituted as it is. And as we are at preſent in our infancy with reſpect to the knowledge of nature, we may aſſure ourſelves, we are likewiſe but in our infancy with reſpect to the power we may acquire of guarding ourſelves againſt the inconveniencies, and ſecuring the advantages of our ſituation.

It is nothing but a ſuperior knowledge of the laws of nature, that gives Europeans the advantage they have over the Hottentots, or the loweſt of our ſpecies. Had theſe people never known Europeans, they could not have formed an idea of any mode of life ſuperior to their own, though it differs but little from that of the brutes. In like manner, ſcience advancing, as it does, with an accelerated progreſs, it may be taken for granted, that mankind ſome centuries hence will be as much ſuperior to us in knowledge, and improvements in the arts of life, as we now are to the Hottentots, though we cannot have any conception what that knowledge, or what thoſe improvements, will be. It is enough for us to ſee that nature is inexhauſtible, that it is a rich mine, in which we ſhall never dig in vain, and that it is open to infinitely more labourers than are now employed in exploring its contents, or in digging for them.

Having been a pretty ſucceſsful adventurer in this great mine, my philoſophical friends in general wonder that I do not confine my attention to it. Their diſſatisfaction with me is ſo great, and I hear of it from ſo many quarters, that I think it right to take ſome opportunity (and a better than the preſent will hardly occur) to make an apology for my conduct, eſpecially to thoſe of my friends by whoſe aſſiſtance I am enabled to give my time to theſe liberal purſuits; being pleaſed to think that my attention to them will be of ſome advantage to ſcience and the public.

In the firſt place, I would obſerve, that I follow my own beſt judgment in devoting my time to what I really apprehend to be the moſt important purſuits, thoſe from which myſelf, and mankind at large, will finally derive the greateſt advantage; and I muſt be allowed to ſay, that the greater variety of objects to which it is evident that I have given attention, muſt qualify me to be a better judge in this caſe than thoſe who cenſure my conduct. Perſons who have only one object of purſuit, never fail to over-rate it, and of courſe to undervalue other things.

I would farther obſerve, that the attention I have given to *theology* (which, by the way, is my original and proper province, and for which I may, therefore, be allowed to have a juſtifiable predilection) does not engroſs ſo much of my time as ſome perſons may imagine. I am particularly complained of at preſent, as having thrown away ſo much time on the compoſition of my *Hiſtory of the Corruptions of Chriſtianity*, and of the *Opinions concerning Chriſt*. But I can aſſure them, and the nature of the thing, if they conſider it, may ſatisfy them, that the time I muſt neceſſarily have beſtowed upon the experiments of which an account is contained in this ſingle volume, is much more than I have given to the *ſix*, of which the above-mentioned works conſiſt, and to all the controverſial pieces that I have written in defence of the former of them. The labour and attention neceſſary to enable me to write ſingle paragraphs in this work, have been more than was requiſite to compoſe whole ſections, or chapters, of the former. In general, during the compoſition of thoſe works, the greateſt part of every day was ſpent in my laboratory, and the evenings and mornings only in reading or writing. Beſides, theſe different ſtudies ſo relieve one another, that I believe I do more in each of them, by applying to them alternately, than I ſhould do, if I gave my whole attention to one of them only.

But my principal defence reſts on the ſuperior dignity and importance of *theological ſtudies* to any other whatever. Every rational being ought to diſtinguiſh, by the greater attention that he gives to them, thoſe objects which are of the greateſt importance to himſelf, and to mankind at large. And certainly, if there be any juſt rule for eſtimating the value of a problem, or query, that is propoſed to us, we muſt think it of infinitely more moment to diſcover whether there be a future, and eſpecially an endleſs life after this, and how to ſecure a happy lot in that future life, than to make the beſt proviſion poſſible for ourſelves in this life, which is the ultimate object of all natural philoſophy. Studies, but remotely connected with that great object, muſt have a dignity and importance infinitely ſuperior to any other. A man muſt never have thought a moment on the ſubject, if he heſitate to give a decided preference in the caſe. To think or act otherwiſe, would be like a man buſying himſelf about farthings, who has large eſtates, or kingdoms, depending, and who ſhould neglect the latter in order to ſecure the former.

All that any philoſophical perſon can pretend to ſay in the caſe, muſt be, that the expectation of a future life is ſo manifeſtly chimerical, that it can never be worth a wiſe man's while to loſe a moment in thinking about it, or to employ his time in any ſtudy relating to it. This I know to be the opinion of many who will read this book, if not this preface. But in this I muſt take the liberty to differ from them, and for reaſons which I ſhall ſubmit to their ſerious conſideration.

Natural phænomena, I agree with them, are unfavourable to any expectation of a future life, and the doctrine of an *immaterial ſoul*, capable of ſubſiſting and acting when the body is in the grave, on which the doctrine of a future ſtate is generally founded, I am as fully perſuaded as they can be, is unauthorized by any natural appearances whatever. My expectation of a future life reſts on another foundation; and, improbable as I acknowledge the doctrine to be, according to the light of nature, it is nevertheleſs ſuch as I firmly believe, on the plaineſt of all evidence; the author of nature having given us an abſolute aſſurance of it, by perſons authorized to ſpeak in his name, and whoſe divine miſſion was proved by ſuch works as no other than the author of nature could have enabled them to perform.

That ſuch works have been performed, and for this important purpoſe, muſt, I apprehend, be true, if there be any truth in hiſtory. And there is no kind of evidence more eaſily ſubjected to a rigorous examination than that which is of the hiſtorical kind, the maxims of which we are every day converſant with.

Now it appears to me, that we muſt either admit the truth of the goſpel hiſtory, which contains an account of the doctrine, miracles, death, and reſurrection of Chriſt (on which the belief of a future life depends) or believe what is infinitely more incredible, viz. that ſeveral thouſand people, preſent at the tranſactions, and who had no motive to believe them without ſufficient evidence, but every motive to turn their eyes from them, or diſbelieve them if they could, ſhould yet, without ſuch evidence, have given the firmeſt aſſent to them, and have entertained ſo little doubt of the extraordinary facts, as to maintain their faith in them at the hazard of every thing dear to them in life, and even chearfully lay down their lives, rather than abandon their faith. Let philoſophers, as ſuch, account for this great *fact*, without admitting more real miracles, and thoſe of a more extraordinary kind, than the belief of chriſtianity requires of me, and I will relinquiſh my preſent faith, dear as it is to me, and join them in expoſing it.

As philoſophers, the queſtion between us is, whoſe faith, ſtrictly ſpeaking, is more agreeable to *preſent appearances*. Whatever we may think of an *author of nature*, and of his attention to it, we equally believe in *the uniformity of the laws of nature*, and that man, whoſe conſtitution is a part of the ſyſtem of nature, was the ſame kind of being two thouſand years ago that he is now; as much as that a horſe of that age, or an oak tree of that age, had the ſame properties with the horſes and oaks of the preſent. Conſequently, whatever was poſſible with reſpect to man in any former period, is equally poſſible now.

But will any man, who gives a moment's attention to the ſubject, ſay that it is even *poſſible* that ſeveral thouſand perſons, in London or Paris, could be made to believe that any man in London or Paris, died and roſe from the dead in their own life-time, that they ſhould perſiſt in this perſuaſion through life, without ſhewing any ſign of inſanity, that they ſhould gain numerous proſelytes to their opinion, though it ſubjected all who embraced it to all kinds of perſecution, and even to death; and that the belief of it ſhould eſtabliſh itſelf againſt all oppoſition, without any perſon being able to detect the impoſition?

Now I apprehend that this might take place more eaſily in London, or in Paris, at this day, than it could have done at Jeruſalem in the time of our Saviour. Human nature could not have been the ſame thing then that we find it to be at preſent, if mankind could have been ſo impoſed upon. This I therefore think abſolutely incredible, and conſequently, as the leſs difficulty of the two, as believing a thing much leſs improbable, I admit the truth of the goſpel hiſtory, the admiſſion of which makes the ſubſequent account of the propagation of chriſtianity (which all hiſtory, and even the preſent ſtate of things proves to be true) perfectly eaſy and natural. Admitting theſe leading facts, all the reſt follows of courſe, and all things came to be as they are without any farther miracle. But real miracles we muſt have ſomewhere, in order to account for the preſent ſtate of things; and if we muſt admit miracles, let them be ſuch as have a *great object*, and not ſuch as have no object at all, but only ſerve to puzzle and confound us.

The hiſtory of the Jews, and the books of the Old Teſtament, furniſh many *facts*, which no hypotheſis beſides that of the divine origin of their religion can explain. Let the philoſopher only admit as *a poſtulatum* that Jews are, and always were, *men*, conſtituted as other men are, and let him not deceive himſelf, by conſidering them as beings of another ſpecies. All I wiſh in this reſpect is, that perſons who pretend to the character of *philoſophers*, would be ſo throughout, and carry the ſame ſpirit into the ſtudy of hiſtory, and of human nature, that they do into their laboratories; firſt aſſuring themſelves, with reſpect to *facts*, and then explaining thoſe facts by reducing them to *general principles* (which, from the uniformity of nature, muſt be univerſally true) and then I ſhall have no doubt of their becoming as firm believers in chriſtianity as myſelf. They will find no other hypotheſis, that can explain ſuch appearances as they cannot deny to be real. Let philoſophers now ſay, whether there be reaſon in this, or not.

I therefore take the liberty, having been led to advance thus much, to addreſs my brother philoſophers on a ſubject equally intereſting to us as *philoſophers*, and as *men*. Do not diſregard a queſtion of infinite moment. Give it that degree of attention to which it is naturally intitled; and eſpecially do not ſo far abandon the ſerious character of *philoſophers*, as to *laugh* where you ought to *reaſon*. At leaſt, do this great ſubject, and yourſelves, the juſtice to conſider the *facts*, and endeavour to frame ſome *hypotheſis* by which to account for them; and do not decide in half an hour, on an inquiry which well deſerves the ſtudy of a great part of your lives.

If I have a ſtronger bias than many other perſons in favour of chriſtianity, it is that which philoſophy gives me. I view with rapture the glorious face of nature, and I admire its wonderful conſtitution, the laws of which are daily unfolding themſelves to our view. It is but little that the life of man permits us to ſee at preſent, and therefore I feel a moſt eager deſire to renew my acquaintance with it hereafter, and to reſume thoſe inquiries with which I am ſo much delighted now, and which muſt be interrupted by death.

Could I imagine that the knowledge of nature would ever be exhauſted, and that we were approaching to a termination of our inquiries, I could more contentedly ſhut my eyes on a ſcene in which nothing more was to be ſeen, or done. But to quit the ſtage at preſent (and I believe the aſpect of things will be exactly ſimilar in any future period of our exiſtence) without the hope of re-viſiting it, would fill me with the deepeſt regret. The general who, like Epaminondas, or Wolfe, dies in the arms of victory, dies with ſatisfaction; but not ſo he that is cut off in the beginning of a doubtful, though promiſing engagement. Thus I feel on the idea of ceaſing to breathe when I have but juſt begun to know what it is that I breathe.

M. Herschell's late diſcoveries in, and beyond, the bounds of the ſolar ſyſtem, the great views that he has given us of the arrangement of the ſtars, their revolutions, and thoſe of the immenſe ſyſtems into which they are formed, are peculiarly calculated to inſpire an ardent deſire of ſeeing ſo great a ſcene a little more unfolded. Such diſcoveries as theſe, give us a higher idea of the value of our being, by raiſing our ideas of the ſyſtem of which we are a part, and, with this, an earneſt wiſh for the continuance of it.

Beſides, *civil ſociety* is but in its infancy, the world itſelf is but very imperfectly known to the civilized inhabitants of it, and we are but little acquainted with the real value of thoſe few of its productions of which we have ſome knowledge, and which we are only beginning to name, and to arrange. How muſt a *citizen of the world* wiſh to know the future progreſs of it.

To have no wiſh of this kind certainly argues a low, an ignoble, and I will ſay, an unphiloſophical mind. I conſider all ſuch perſons, how ſuperior ſoever they may be to myſelf in other reſpects, with pity and concern. They would have unſpeakably more ſatisfaction in their philoſophical purſuits, if they carried them on with the views of things that I have. It has been juſtly obſerved, that great views indicate, and indeed conſtitute, great minds. What elevation of mind, then, would the proſpects of the chriſtian, add to thoſe of the philoſopher*.

With men of reflection this apology for my conduct will, I doubt not, be admitted as ſatisfactory; and till I hear better reaſons than have yet been offered to me for changing my conduct, I ſhall continue to give my attention to my different purſuits, according to my own ideas of their reſpective im-

---

* If any of my philoſophical friends ſhould be induced, by what I have here urged, to look into my *theological writings*, I would take the liberty to recommend to them my *Letters to a Philoſophical Unbeliever*, the *Inſtitutes of natural and revealed Religion*, and the *Hiſtory of the Corruptions of Chriſtianity*, eſpecially the *Concluſion*, part I. relating to Mr. GIBBON, who has declined engaging in the diſcuſſion I there propoſed to him. If they wiſh to ſee more particularly in what manner chriſtianity came to be encumbered with the doctrine of *the trinity*, which has been the foundation of one of the greateſt objections to it, I would further refer them to my *Hiſtory of early opinions concerning Chriſt*, where they will ſee it traced to its proper ſource in the Platonic philoſophy, and where it is proved that the primitive chriſtian church was unqueſtionably unitarian.

portance;

portance; and my friends have no reaſon to fear that I ſhall neglect *philoſophy*. It has, perhaps, but too ſtrong charms for me. I ſhall endeavour, however, to keep it in its proper place, and not ſo much attach myſelf to the ſtudy of the laws which govern *this* world, as to loſe ſight of the ſubſerviency of this world, and of all things in it, to *another* and a better; in which I hope to reſume theſe pleaſing philoſophical purſuits, and to ſee, in a comprehenſive view, thoſe detached diſcoveries which we are now making here.

At preſent all our *ſyſtems* are in a remarkable manner unhinged by the diſcovery of a multiplicity of *facts*, to which it appears difficult, or impoſſible, to adjuſt them. We need not, however, give ourſelves much concern on this account. For when a ſufficient number of new facts ſhall be diſcovered (towards which even imperfect hypotheſes will contribute) a more *general theory* will ſoon preſent itſelf; and perhaps to the moſt incurious and leaſt ſagacious eye. Thus, when able navigators have, with great labour and judgment, ſteered towards an undiſcovered country, a common ſailor, placed at the maſt head, may happen to get the firſt ſight of the land. Let us not, however, contend about *merit*, but let us all be intent on forwarding the *common enterprize*, and equally enjoy any progreſs we may make towards ſucceeding in it; and above all, let us acknowledge the guidance of that Great Being, *who has put a ſpirit in man, and whoſe inſpiration giveth him underſtanding.*

In this volume I have continued to expreſs the degree of purity in air as in the former; ſaying, for inſtance, that air is of the ſtandard of 1.2 or 1.4, when one meaſure of the air to be examined, and one of nitrous air, preſently after being mixed together, occupy the ſpace of one meaſure, and two, or four-tenths of a meaſure. But if the air be called *dephlogiſticated*, it is always to be underſtood that *two* equal meaſures of nitrous air were uſed. The *weights* mentioned in this volume are *ounces* and *grains*, the ounces being Engliſh troy weight, containing 480 grains. I have dropped the uſe of the *penny-weight* (*dwt.*) which is twenty-four grains, [redacted], in conſequence of finding that it occaſioned ſome difficulty to foreigners.

An

An explanation of the Figures in the plate prefixed to this volume.

Fig. I. is a view of the apparatus with which the principal experiments relating to the ſeeming converſion of water into air were made. It conſiſts of *an earthen veſſel*, the bulb of which, containing moiſtened clay, is fixed in the inſide of *a glaſs veſſel*, through which the heat of a burning lens may be thrown upon it; while the inſide has a communication with a baſon of water, or mercury, in which veſſels may be placed to receive the air that is forced through the body of the earthen veſſel; while the water, or mercury, in the *baſon* in which the glaſs veſſel ſtands, riſes within it, to ſupply the place of that air.

Fig. II. Shews the diſpoſition of the apparatus by which ſteam is tranſmitted through a red-hot tube, containing iron, &c. with *a worm tub* to collect the ſuperfluous water, &c. and a veſſel to receive the air that is produced. This veſſel is here drawn very ſmall, that it might not take up much room in the plate; but I have generally uſed a large trough for this purpoſe, and jars of conſiderable ſize to receive the air. Inſtead of

the ſmall *furnace* to heat the water, &c. I now uſe one of *Mr. Argand's lamps*, which is, on ſeveral accounts a very valuable addition to a chemical apparatus. Fig. VI. repreſents the method of receiving the air in this proceſs under a funnel, fixed in a trough of water, which may be uſed when large balloons are filled, and when no account is taken of any water that is condenſed in the proceſs.

Fig. III. repreſents the contrivance, mentioned page 135, by means of which all the air that was driven out of a veſſel, by pouring mercury into it, was obliged to paſs through a thin glaſs veſſel, in which it might depoſit the moiſture, &c. it contained.

Fig. IV. repreſents a large glaſs balloon, in which inflammable air, iſſuing from the orifice of a ſmall tube, burns like a candle, while the water produced by the proceſs is collected in the inſide of it.

Fig. V. repreſents a ſtrong cylindrical glaſs veſſel, in which inflammable and dephlogiſticated air may be fired. It is furniſhed with a wooden cap, firmly cemented to the open end of it, and cloſed with a ſcrew, and two iron wires are inſerted at the top of it, between which an electric ſpark can be taken.

THE

CONTENTS

SECTION I.

EXPERIMENTS *relating to Phlogiſton* 1

SECTION II.

Experiments relating to the ſeeming Converſion of Water into Air - 29

The ſame Experiments continued 64

SECTION III.

Experiments and Obſervations relating to Air and Water - - 70

Poſtſcript - - 119

SECTION IV.

Farther Obſervations relating to the Compoſition of Water - - 125

SECTION V.

Of the Production of Inflammable Air from different Subſtances - 145

SECTION VI.

Of ſulphurated Inflammable Air 153

SECTION VII.

The Analyſis of different Kinds of Inflammable Air - - - 162

SECTION VIII.

The Analyſis of Alkaline Air 189

SECTION IX.

Of the Converſion of Alkaline Air into Inflammable Air - - 197

SECTION X.

Of the Connexion between Alkaline Air, Volatile Alkali, and Inflammable Air 203

SECTION XI.

Of the Charcoal of Metals 207

SECTION XII.

Of Air extracted from mineral Subſtances 215

SECTION XIII.

Experiments on Charcoal 237

SECTION XIV.

Of the conſtituent Principles of Fixed Air 248

SECTION XV.

Of the Change made in Fixed Air by the electric Spark - - 273

SECTION XVI.

Of the Quantity of Fixed Air expelled from various Subſtances 283

SECTION XVII.

Experiments relating to Dephlogiſticated Air 290

SECTION XVIII.

Of the conſtituent Principles of Nitrous Air 299

SECTION XIX.

Of Nitrous Air in general 309

SECTION XX.

Experiments relating to Dephlogiſticated Nitrous Air - - 315

SEC-

SECTION XXI.

Experiments with Electricity 338

SECTION XXII.

Of the Influence of Light on Vapour of Spirit of Nitre - - 342

SECTION XXIII.

Experiments on the Action of Steam upon ſeveral heated Subſtances 347

SECTION XXIV.

Experiments relating to Iron 355

SECTION XXV.

Experiments on Finery Cinder 372

SECTION XXVI.

Of Air acting through a Bladder 377

SECTION XXVII.

Miſcellaneous Experiments 385
1. *Of the Influence of Water on Phlogiſticated Air* - - ibid.

2. *Of Air produced more or leſs rapidly* 389

3. *Of different Kinds of Air remaining diffuſed through each other without uniting* 390

4. *Of the Ice of Water impregnated with Vitriolic Acid Air* 392

5. *Of the Precipitate from the Solution of Metals by Heat* - - ibid.

6. *Of heating Iron in fluor acid Air* 394

7. *Of the Solution of Precipitate per ſe in Spirit of Salt* - - ibid.

8. *Of Air extracted from the Water that is uſeful in fertilizing Meadows* 395

9. *Of Water heated in Gun Barrels welded up* - - 397

10. *Of the Solution of Iron in phlogiſticated and dephlogiſticated nitrous Acid* 398

SECTION XXVIII.

Obſervations relating to Theory 400

SECTION XXIX.

Experiments made ſince the Sections in this Work to which they relate were printed off 427

APPENDIX 439

No. I. *A Letter from* JAMES KEIR, ESQ. F.R.S. *relating to a Metallic Pyrophorus* ibid.

No. II. *A Letter from Mr.* THOMAS HENRY, F.R.S. *concerning the Phlogiſton of Alkali* - - 446

ERRATA, &c.

N. B. (*b*) ſignifies from the bottom of the page.

Page 152. l. 8. *for* ſeventeen, *read* ſixteen.

———— l. 10. (*b*) *for* three times, *read* between four and five times.

—— 232. l. 9. *for* woad, *read* wad.

—— 291. l. 4. *for* water, *read* of water.

—— 303. l. 8. (*b*) *dele* yet.

—— 319. l. 10. (*b*) *for* crucible, *read* retort.

—— 328. l. 1. (*b*) *add* as may be inferred from part of it being converted into pure air by the electric ſpark.

—— 354. l. 9. *read* manganeſe, which I had from the glaſs houſe.

—— 375. l. 11. *read* of the greateſt part of its own phlogiſton.

ERRATUM.

Vol. II. p. 74. l. 4. (*b*) *for* one ounce meaſure, *read* one third.

OBSERVATIONS

RELATING TO

VARIOUS BRANCHES

OF

NATURAL PHILOSOPHY.

SECTION I.

Experiments relating to Phlogiſton[*].

THERE are few ſubjects, perhaps none, that have occaſioned more perplexity to chemiſts, than that of *phlogiſton*, or, as it is ſometimes called, *the principle of inflammability*. It was the great diſcovery of STAHL, that this principle, whatever it be, is transferable from one ſubſtance to another, how different ſoever in their other properties, ſuch as ſulphur, wood, and all the metals, and therefore is the ſame thing in them all. But what has given an air of myſtery to this ſubject, has been that it was imagined, that this principle, or ſubſtance, could not be exhibited except in combination with other ſubſtances, and could not be made to aſſume ſeparately either a fluid or ſolid form. It was alſo aſſerted by ſome, that phlogiſton was ſo far from adding to the weight of bodies, that the addition of it made them really lighter than they were before; on which account they choſe to call it *the principle of levity*. This opinion had great patrons.

---

[*] This ſection is copied from the Philoſophical Tranſactions, vol. lxxiii. p. 399. with the addition of ſome notes.

Of late it has been the opinion of many celebrated chemiſts, Mr. LAVOISIER among others, that the whole doctrine of phlogiſton had been founded on miſtake, and that in all caſes in which it was thought that bodies parted with the principle of phlogiſton, they in fact loſt nothing, but on the contrary acquired ſomething; and in moſt caſes an addition of ſome kind of air; that a *metal*, for inſtance, was not a combination of two things, *viz*. an *earth* and *phlogiſton*, but was probably a ſimple ſubſtance in its metallic ſtate; and that the calx is produced not by the loſs of phlogiſton, or of any thing elſe, but by the acquiſition of air.

The arguments in favour of this opinion, eſpecially thoſe which are drawn from the experiments that Mr. LAVOISIER made on mercury, are ſo ſpecious, that I own I was myſelf much inclined to adopt it. My friend Mr. KIRWAN, indeed, always held that phlogiſton was the ſame thing with inflammable air; and he has ſufficiently proved this from many experiments and obſervations, my own, as well as thoſe of others. I did not, however, accede to it till I diſcovered it by direct experiments, made with general and indeterminate views, in order to aſcertain ſomething concerning a ſubject which had given myſelf and others ſo much trouble.

I began with repeating the experiments in which I had found that inflammable air, made red-hot in flint glaſs tubes, gave them a black tinge, and was in a great meaſure abſorbed, which I had diſcovered to be owing to the calx of lead in the glaſs, attracting phlogiſton from the inflammable air. As the quantity of air in theſe tubes was very ſmall, though I gave it as my opinion, that the reſiduum in one of the proceſſes, was phlogiſticated air, becauſe I perceived no marks of accenſion on preſenting to it the flame of a ſmall candle; I was not, on recollection, ſatisfied with this concluſion, and was deſirous of repeating the experiment with more care, eſpecially as, in one of the above-mentioned experiments, I found only a very ſmall bubble of the inflammable air in the tube in which it had been heated.

I found, however, great difficulty in repeating theſe experiments; and the quantity of inflammable air operated upon in them, is neceſſarily ſo ſmall, that the reſult is always liable to much uncertainty. I thought, therefore, that throwing the focus of a burning lens upon a quantity of pounded flint glaſs, ſurrounded with inflammable air, or rather on the calx of lead alone, in the ſame circumſtances, would be a much eaſier experiment, and might bring me nearer to my object; and on making the experiment it immediately anſwered far beyond my expectation.

For this purpoſe, I put upon a piece of a broken crucible (which could yield no air) a quantity of minium, out of which all air had been extracted; and placing it upon a convenient ſtand, introduced it into a large receiver, filled with inflammable air, confined by water. As ſoon as the minium was dry, by means of the heat thrown upon it, I obſerved that it became black, and then ran in the form of perfect lead, at the ſame time that the air diminiſhed at a great rate, the water aſcending within the receiver. I viewed this proceſs with the moſt eager and pleaſing expectation of the reſult, having at that time no fixed opinion on the ſubject; and therefore I could not tell, except by actual trial, whether the air was decompoſing in the proceſs, ſo that ſome other kind of air would be left, or whether it would be abſorbed *in toto*. The former I thought the more probable, as, if there was any ſuch thing as phlogiſton, inflammable air, I imagined, conſiſted of it, and ſomething elſe. However, I was then ſatisfied that it would be in my power to determine, in a very ſatisfactory manner, whether the phlogiſton in inflammable air had any *baſe* or not, and if it had, what that baſe was. For ſeeing the metal to be actually revived, and that in a conſiderable quantity, at the ſame time that the air was diminiſhed, I could not doubt, but that the calx was actually imbibing ſomething from the air; and from its effects in making the calx into metal, it could be no other than that to which chemiſts had unanimouſly given the name of *phlogiſton*.

Before this firſt experiment was concluded, I perceived, that if the phlogiſton in inflammable air had any baſe, it muſt be very inconſiderable: for the proceſs went on till there was no more room to operate without endangering the receiver; and examining, with much anxiety, the air that remained, I found that it could not be diſtinguiſhed from that in which I began the experiment, which was air extracted from iron by oil of vitriol. I was, therefore, pretty well ſatisfied that this inflammable air could not contain any thing beſides phlogiſton; for at that time I reduced about 45 ounce meaſures of the air to five.

In order to aſcertain a fact of ſo much importance with the greateſt care, I afterwards carefully expelled from a quantity of minium all the phlogiſton, and every thing elſe that could have aſſumed the form of air, by giving it a red heat when mixed with ſpirit of nitre; and immediately uſing it in the manner mentioned above, I reduced 101 ounce meaſures of inflammable air to *two*. To judge of its degree of inflammability, I preſented the flame of a ſmall candle to the mouth of a phial filled with it, and obſerved, that it made thirteen ſeparate exploſions, though weak ones (ſtopping the mouth of the phial with my finger after each exploſion) when freſh made inflammable air, in the ſame circumſtances, made only fourteen exploſions, though ſtronger ones.

After this experiment I could not heſitate to conclude*, that this inflammable air went totally, and without decompoſition, into the lead which I formed at that time; and if the neceſſary circumſtances of the experiment be conſidered, it will be thought extraordinary that, even admitting this, the reſult ſhould be ſo deciſively clear in favour of it: for, in the firſt place, the greateſt care muſt be uſed to expel all air from the minium, and it muſt be uſed before it can have attracted any from the atmoſphere; and in the next place, the water alſo (a conſiderable quantity of which muſt be uſed, and which will alſo be heated in the proceſs) ſhould be made as free from air as poſſible. In theſe circumſtances, had I found the ſmall reſiduum, of two ounce meaſures from 101, to have been phlogiſticated or fixed air, I ſhould not have been diſappointed; and it would not have prevented my concluding that *phlogiſton* was the ſame thing with *inflammable air*, contained in a combined

---

* In this concluſion, I overlooked one obvious conſideration, viz. that water, or any thing ſoluble in water,

---

might be the baſis of inflammable air. All that could be abſolutely inferred from the experiment was, that this baſis could not be any thing that was capable of ſubſiſting in the form of *air*. It will be ſeen, that I afterwards made the experiment with the air confined by mercury.

combined ſtate in metals, juſt as fixed air is contained in chalk and other calcareous ſubſtances; both being equally capable of being expelled again in the form of air.

Afterwards uſing a calx of lead, which had been prepared in the ſame manner with the former, but which had remained ſome weeks expoſed to the air, I found, that when by uſing it I had reduced 150 ounce meaſures of inflammable air to 10, this reſiduum was phlogiſticated air. But examining this calx ſeparately, I found that it gave, by heat in a glaſs veſſel, a conſiderable quantity of phlogiſticated air.

I muſt obſerve, that the minium ſhould not be reduced to a perfectly compact *glaſs of lead*; for then it would be too refractory to be eaſily revived by this proceſs. Making uſe of ſome of it, I found that I could only melt it; but that a copious black fume came from it, and coated the inſide of the receiver: an experiment which I ſhall repeat and reconſider. I muſt alſo obſerve, that the lead which I procured in the above-mentioned proceſs was not to be diſtinguiſhed from any other lead, and that the inflammable air was all procured from iron by oil of vitriol.

When I made uſe of inflammable air from wood, I found, that though I was able to reduce minium with it, it was effected with more time and difficulty. Forty ounce meaſures of this kind of inflammable air I reduced to 25; after which I found that the heat of the lens produced only *glaſs of lead*, and no *metal*. The air was ſtill, however, inflammable; and there was a ſmall mixture of fixed air in it. This kind of inflammable air, which burns with a lambent flame, I have ſome reaſon to think, conſiſts of an intimate union of fixed air with that which is of the *exploſive* kind extracted from metals. The reſult of thoſe experiments which I made with that kind of inflammable air which is collected in the proceſs for making phoſphorus, and which burns with a lambent yellow flame, was ſimilar to thoſe which I made with inflammable air from wood, which burns with a lambent white flame.

Having had this remarkable reſult with inflammable air, I immediately tried all the other kinds of air in the ſame manner; but in none of them did I procure any thing from the minium beſides glaſs of lead, except in alkaline air, and vitriolic acid air. In fixed air, nitrous air, phlogiſticated air, marine acid air, fluor acid air, as alſo in common and dephlogiſticated air, I got no *metal* at all. In vitriolic acid air there was but a ſmall quantity of lead produced, and I have obſerved that this kind of air imparts a certain portion of phlogiſton to common air, rendering it in ſome meaſure phlogiſticated, though by no means in ſo great a degree as nitrous air.

Though nitrous air and phlogiſticated air certainly contain phlogiſton, they appear by theſe experiments to hold it too obſtinately to part with it to minium in this proceſs, notwithſtanding nitrous air quits it ſo readily to reſpirable air. I would obſerve, that there were ſome peculiar appearances in the experiments I made to revive the calx of lead in theſe kinds of air in which the attempt did not ſucceed; but I muſt repeat the experiments, and note the appearances more accurately, before I report them.

In alkaline air lead ſeems to be formed from the minium as readily as in inflammable air, and indeed I thought rather more ſo; and this is a remarkable confirmation and illuſtration of thoſe experiments, in which, by taking the electric ſpark in a quantity of alkaline air, I converted it into three times as much pure inflammable air; an experiment which, on account of the extraordinary nature of it, I have repeated many times ſince I firſt publiſhed the account of it, and always with the ſame reſult.

This experiment alſo throws ſome light upon thoſe in which, by ſuper-phlogiſticating iron with nitrous air, I produced a ſtrong ſmell of volatile alkali; an experiment which I have alſo frequently repeated with the ſame reſult. The reviving of lead in alkaline air may alſo help us to conceive how all *acids* ſhould have an affinity both to *phlogiſton* and to *alkalies*, which have hitherto appeared to be things ſo very different from each other; ſince, from theſe experiments, it is probable that one of them is ſome modification of the other, or a combination of ſomething elſe with the other. To trace the connexion between the alkaline and inflammable principles, is a curious ſubject; and from theſe hints it may, perhaps, not be very difficult to proſecute it to advantage. It is evident, however, from the following experiments, that alkaline air is the compound, and inflammable air, or phlogiſton, the more ſimple ſubſtance of the two.

From 5½ ounce meaſures of alkaline air I got, by means of litharge, 17 grains of lead, beſides ſome that was diſſolved in the mercury, by which the air was confined. There remained 2½ ounce meaſures, which appeared to be phlogiſticated air, and to have no fixed air in it. At another time, in eight ounce meaſures of alkaline air I got 15 grains of lead, beſides what was diſſolved in the mercury, which ſeemed to be a good deal in proportion to it. It was obſervable, that there remained in this proceſs 3½ ounce meaſures

ſures of phlogiſticated air, without any mixture of fixed air in it, though the maſſicot which I uſed at this time gave by heat only a good deal of pretty pure fixed air. Theſe experiments with alkaline air well deſerve to be reſumed, and I ſhall not fail to do it at a proper opportunity.

Having thus produced *lead* in inflammable air, I proceeded in my attempts to revive other metals from their calces by the ſame means; and I ſucceeded very well with tin, biſmuth, and ſilver; tolerably well with copper, iron, and regulus of cobalt; but not at all with regulus of antimony, regulus of arſenic, zinc, or the metal of manganeſe.

I was deſirous alſo of aſcertaining by this means the *quantity* of phlogiſton that enters into the compoſition of the ſeveral metals; but in this I found more difficulty than I had expected; and this aroſe chiefly from the allowance that was to be made for the inflammable air which entered into that part of the calx which was only partially revived; and it was not eaſy to revive the whole of any quantity of calx completely.

After many trials, I think I may venture to ſay, that an ounce of *lead* abſorbs 100 ounce meaſures of inflammable air, or perhaps ſomething more; for in one reſult it ſeemed to have imbibed in the proportion of 108 ounce meaſures.

An ounce of *tin* abſorbs inflammable air in the proportion of 377 ounce meaſures to the ounce. An ounce of copper from verditer abſorbed 403 ounce meaſures; from a ſolution of blue vitriol, precipitated by ſalt of tartar, and afterwards made red-hot with ſpirit of nitre, 640; but from blue vitriol itſelf 909 ounce meaſures. In this caſe, however, much of the inflammable air went to the formation of the vitriolic acid air, the ſmell of which was very perceivable in the courſe of the experiment. The copper that I made in this way was brittle, and therefore ſeemed not to be perfectly metalized; but being fuzed with borax, it became perfect copper, and, as I think, without any loſs of weight.

Biſmuth abſorbed inflammable air in the proportion of 185 ounce meaſures to the ounce.

The calx I uſed was a precipitate from the ſolution of this metal in ſpirit of nitre.

Iron I got from a precipitate of a ſolution of green vitriol by ſalt of tartar, moiſtened with ſpirit of nitre, and expoſed to a red heat. This calx abſorbed in the proportion of 890 ounce meaſures of the inflammable air to an ounce of iron, which was in the form of a black powder; but to all appearance as much attracted by the magnet as iron filings. But it could not be expected, that perfect iron, containing its full proportion of phlogiſton, ſhould be produced in this manner, ſince inflammable air may be expelled from perfect iron in this very proceſs *.

Silver I evidently revived from a ſolution of it in ſpirit of nitre precipitated by ſalt of of tartar, and alſo from *luna cornea*. A quantity of this laſt ſubſtance abſorbed 23 ounce meaſures of inflammable air; but I could not get any calx of ſilver free from ſmall grains of the perfect metal, which was eaſily diſcovered by a magnifier, and therefore I could not aſcertain the quantity of inflammable air abſorbed by it.

---

* I have ſince found that inflammable air cannot be expelled from iron by heat, without ſome moiſture, which therefore ſeems neceſſary to its conſtitution.

Small grains of regulus of *cobalt* I produced from zaffre, and inflammable air was abſorbed; but I did not eſtimate the quantity.

A quantity of *mangeneſe* abſorbed 7 ounce meaſures of inflammable air; but I could not perceive any thing in it which had the appearance of metal. But I imagined I had not heat enough for the purpoſe, and mixing with it ſome calcined borax, I repeated the experiment, when there was again an evident abſorption of air, and in the courſe of that experiment, I once thought that I did perceive a ſmall globule of metal.

Zinc and *arſenic* were only ſublimed in this proceſs. The ſame was the caſe with the glaſs of *antimony*; but the experiment was attended with this peculiar circumſtance, that when the glaſs was melted in inflammable air, it formed itſelf into needlelike cryſtals arranged in a very curious manner; and I could not produce that appearance in other kinds of air.

Inflammable air being clearly imbibed by the calces of metals, and thereby reviving them, is a ſufficient proof of its *containing* what has been called phlogiſton; and its being abſorbed by them *in toto*, without decompoſition, is a proof of its being nothing beſides *phlogiſton in the form of air*, unleſs there ſhould be ſomething ſolid depoſited from it at the ſame time that the proper phlogiſtic part of it is abſorbed. With reſpect to this, I can only ſay that, in the courſe of the experiments, I did not perceive any thing of the kind: for though in ſome of the proceſſes there was a black ſmoke produced, in others I could perceive nothing but part of the calx ſubliming, and clouding the glaſs. On this account, however, I could not pretend to aſcertain the weight of the inflammable air in the calx, ſo as to prove that it had acquired an addition of weight by being metalized, which I often attempted. But were it poſſible to procure a perfect calx, no part of which ſhould be ſublimed and diſperſed, by the heat neceſſary to be made uſe of in the proceſs, I ſhould not doubt but that the quantity of inflammable air imbibed by it would ſufficiently add to its weight.

Beſides the formation of metals from their calces, I had other proofs, and of a nature ſufficiently curious, of inflammable air containing phlogiſton, though perhaps not ſufficiently concluſive with reſpect to its being wholly and ſimply phlogiſton itſelf. Thus, by means of it, I was able to make *phoſphorus*, *nitrous air*, *liver of ſulphur*, and *ſulphur* itſelf, in all of which phlogiſton is acknowledged to be a principal ingredient.

Throwing the focus of the lens upon a quantity of that glaſſy matter which is made from calcined bones by oil of vitriol in inflammable air, ſome of it was abſorbed, and all the inſide of the receiver was covered with an orange-coloured ſubſtance, which had a ſtrong ſmell of phoſphorus. I then wanted ſun-ſhine to continue the experiment; but I was ſatisfied that there was ſufficient proof of phoſphorus being actually formed in this manner. With alkaline air I ſucceeded much better.

In 2 ½ ounce meaſures of this air, I produced, from the glaſſy matter mentioned above, two grains of phoſphorus in one maſs, the veſſel being only filled with white fumes during the proceſs. One-fourth of the bulk of the air remained, and this was inflammable, burning with a yellow lambent flame, exactly like that which is produced in the proceſs for making phoſphorus.

That nitrous air contains phlogiſton is ſufficiently evident, if there be any ſuch thing as phlogiſton: and I have farther proved, that it contains as much phlogiſton, in proportion to its bulk, as inflammable air itſelf. I have now, however, the farther ſatisfaction to be able to make nitrous air from its two conſtituent principles, *viz.* nitrous vapour and inflammable air. The moſt eaſy proceſs for this purpoſe is, to throw a ſtream of nitrous vapour into a large phial previouſly filled with inflammable air. In this manner nitrous air is inſtantly formed, and in great quantities; but as this nitrous vapour is produced by the rapid ſolution of biſmuth in ſpirit of nitre, which at the ſame time produces a quantity of nitrous air, the experiment is not quite unexceptionable. I therefore attempted the ſame thing in the following manner.

Taking a quantity of what I have called a *nitrated calx* of lead, which I firſt produced by uniting nitrous vapour to minium (in conſequence of which, from being a red and powdery ſubſtance, it becomes white, compact, and brittle) I placed it upon a ſtand, in a receiver filled with inflammable air, and throwing the focus of the lens upon it, there was a diminution of the inflammable air, which amounted to about two-thirds of the whole, and during this time lead was revived from the calx. After this there was no more diminution of the air, or revival of the calx: and then examining what remained of the air, I found it to be all ſtrongly nitrous: and, from the circumſtances in which it was produced, it muſt have been formed from the nitrous vapour contained in the calx, and the inflammable air in the receiver.

In order to aſcertain the purity of this nitrous air, I mixed it with an equal quantity of common air, and found that they occupied the ſpace of 1,32 meaſures. Freſh nitrous air made in the uſual way, and mixed with common air in the ſame proportion, occupied the ſpace of 1,26. This difference aroſe not from any impurity in the nitrous air, but from the mixture of the dephlogiſticated air, which is alſo expelled from this calx by heat.

Liver of ſulphur was procured by throwing the focus of the lens upon vitriolated tartar in inflammable air, and it appeared to be perfectly well formed.

Laſtly, to produce *ſulphur*, I threw the focus of the lens on a quantity of oil of vitriol, contained in an hollow earthen veſſel, and evaporated it to dryneſs in a receiver filled with inflammable air; in conſequence of which the inſide of the receiver acquired a whitiſh incruſtation, which when warmed had a ſtrong ſmell of ſulphur; and repeating the proceſs in the ſame receiver, I was able, this ſecond time, to ſcrape off enough of the matter to put on a piece of hot iron, and to produce the genuine blue flame, as well as the peculiar ſmell of ſulphur.

I ſhall conclude theſe obſervations on phlogiſton with two articles; one of which ſeems to contradict an eſtabliſhed maxim among chemiſts; and the other a former opinion of my own.

It is generally ſaid, that charcoal is indeſtructible, except by a red heat in contact with air. But I find that it is perfectly deſtructible, or decompoſed, *in vacuo*, and by the heat of a burning lens almoſt wholly converted into inflammable air; ſo that nothing remains beſides an exceedingly ſmall quantity of white aſhes, which are ſeldom viſible, except when, in very ſmall particles, they happen to croſs the ſun-beam, as they fly about within the receiver. It would be impoſſible to collect or weigh them; but, according to appearance, the aſhes thus produced from many pounds of wood, could not be ſuppoſed to weigh a grain. The great weight of aſhes produced by burning wood in the open air, ariſes from what is attracted by them from the air. The air which I get in this manner is wholly inflammable, without

out the leaſt particle of fixed air in it. But, in order to this, the charcoal muſt be perfectly well made, or with ſuch a heat as would expel all the fixed air which the wood contains; and it muſt be continued till it yield inflammable air only, which, in an earthen retort, is ſoon produced.

Wood, or charcoal, is even perfectly deſtructible, that is, reſolvable into inflammable air, in a good earthen retort, and a fire that would about melt iron. In theſe circumſtances, after all the fixed air had come over, I have ſeveral times continued the proceſs during a whole day, in all which time inflammable air has been produced equably, and without any appearance of a termination. Nor did I wonder at this, after ſeeing it wholly vaniſh into inflammable air *in vacuo*. A quantity of charcoal made from oak, and weighing about an ounce, generally gave me about five ounce meaſures of inflammable air in twelve minutes*.

---

*Notwithſtanding theſe *facts*, it will appear from my ſubſequent experiments, that water was neceſſary to the the formation of this inflammable air from *wood* as well as of that from *iron*.

The ſecond article that I ſhall now mention, affords an indiſputable proof of the generation of fixed air from dephlogiſticated air and phlogiſton, or inflammable air. I have ſeveral times given it as my opinion, that fixed air is a *factitious ſubſtance*, and a modification of the nitrous and vitriolic acids, my former experiments greatly favouring that concluſion; but that it was compoſed of dephlogiſticated air and phlogiſton, though maintained by my friend Mr. KIRWAN, I was far from being ſatisfied with, till I was forced to conſent to his proof of it from my own former experiments, and gave him leave to mention it, as he has done in his late excellent paper on ſalts. But I have lately had two direct proofs of it by experiment.

The firſt was when, in repeating a beautiful experiment, firſt made by Dr. INGENHOUSZ, but with ſome variation, I was firing ſome ſhavings of iron in dephlogiſticated air confined by mercury, by means of a burning lens. In this way I quickly fired the iron, and it burned away in a very pleaſing manner.

ner. But what ſtruck me moſt was, that, of the air that remained, a conſiderable portion was fixed air, though in the receiver I had nothing but the pureſt dephlogiſticated air, together with the iron, which could only give inflammable air. I would obſerve, that the melted iron formed itſelf into large balls, which appeared to be a mere *ſlag* or *glaſs*[*], and was no longer iron.

Afterwards, to put this hypotheſis concerning the conſtituent principles of fixed air to a more direct proof, I mixed iron filings, which give only inflammable air, with red precipitate, which I found to give nothing but the pureſt dephlogiſticated air; and when I heated them in a coated glaſs retort, they gave a great quantity of fixed air, in ſome portions of which, nineteen-twentieths were abſorbed by lime-water; but the reſiduum was inflammable. However, when I mixed with the red precipitate a quantity of powdered charcoal, which I had found to give only inflammable air, the fixed air produced from it was ſo pure, that only one-fortieth part of it remained unabſorbed by water; ſo that this fixed air was as pure as that which is generally procured from chalk by oil of vitriol.

---

[*] The nature of what is here called *ſlag* or *glaſs*, will appear in the ſubſequent experiments; being the ſame that is called *ſcales of iron*, and alſo *finery cinder*, or the calx of iron combined with water.

It appeared, in ſome of theſe experiments, that three ounce meaſures of dephlogiſticated air go into the compoſition of two ounce meaſures of fixed air. For one ounce of this red precipitate gave 60 ounce meaſures of dephlogiſticated air; and when mixed with two ounces of iron filings, it gave about 40 ounce meaſures of fixed air that were actually abſorbed by water, beſides a reſiduum that was inflammable*. I had the ſame proportion when I uſed half an ounce of each of the materials. But when I uſed one ounce of each, I got only 20 ounce meaſures of fixed air, including the reſiduum. At other times

---

* It muſt be obſerved, however, that part of the fixed air is always imbibed by the water in which it is firſt received. Otherwiſe, in this experiment, the fixed air would have weighed no more than the dephlogiſticated air in the compoſition of it, ſo that nothing would be left for the inflammable air.

I had different proportions with different quantities of iron filings and charcoal.

I cannot conclude theſe obſervations without taking notice, how very valuable an inſtrument in philoſophy is a good burning lens. This muſt have been perceived in many of my former experiments, but more eſpecially in theſe. By no other means can heat be given to ſubſtances *in vacuo*, or in any other kind of air beſides atmoſpherical; and without ſome method of doing this, no ſuch experiments as theſe can poſſibly be made. I therefore congratulate all the lovers of ſcience on the ſucceſsful attempt of Mr. PARKER to execute ſo capital an inſtrument as he has done of this kind. Such ſpirited and generous exertions reflect honour on himſelf, and on our country. It is only to be wiſhed, that we could have lenſes of a ſmaller ſize (*viz.* from 12 to 18 inches diameter) made tolerably cheap, ſo that they might be in more common uſe. All my experiments were made with one of 12 inches in diameter.

SECTION II.

*Experiments relating to the ſeeming Converſion of Water into Air**.

SINCE many perſons have expreſſed a wiſh to be acquainted with the experiments I have lately made, which at firſt ſeemed to favour the idea of a *converſion of water into air*, but which terminated in the diſcovery of a fact, in my opinion, ſtill more extraordinary, I ſhall ſubmit to the Royal Society the reſult of the obſervations I have already made; though, as yet, I have by no means been able to ſatisfy myſelf ſo fully as I could wiſh with reſpect to ſome particulars connected with the ſubject. All the *facts* which I ſhall ſtate may be depended upon; but it is probable, that different perſons may draw different *concluſions* from them; and to mere *opinions*, I have never ſhewed myſelf much attached.

---

* This ſection is taken from the Philoſophical Tranſactions, vol. lxxiii. p. 414.

Having formerly obſerved ſeveral remarkable changes in fluid ſubſtances, in conſequence of long expoſure to heat in glaſs veſſels hermetically ſealed (of which an account may be ſeen in the fourth volume of my Philoſophical Obſervations) I then formed a deſign of expoſing all kinds of ſolid ſubſtances to great heats, in a ſimilar ſtate of confinement; and for that purpoſe provided myſelf with a caſt-iron veſſel, which I could cloſe at one end, like a digeſter, and of ſuch a length, that one of the ends might be red-hot, while the other was ſufficiently cool to be handled. To this end there was a cock connected to a tube, by means of which I could let off ſteam, or air, in any period of the proceſs.

I imagined, that when ſubſtances, conſiſting of parts ſo volatile as to fly off before they had attained any conſiderable degree of heat, in the uſual preſſure of the atmoſphere, were compelled to bear great heats under a greater preſſure, they might aſſume new forms, and undergo remarkable changes, ſimilar to what we may ſuppoſe to be the caſe within the bowels of the earth, where, by means of ſubterraneous fires, various ſubſtances bear great heats under very great preſſures.

I have had this inſtrument ſome years; but it was ſo ill conſtructed, that I could not make the uſe of it that I had originally intended. I therefore lately fitted up ſome gun-barrels in the ſame manner, and made my firſt experiment with lime-ſtone; expecting, that when the fixed air, and other volatile matters, that might be contained in it, ſhould be compelled to bear a red-heat, without a poſſibility of making their eſcape, the ſubſtance itſelf might undergo ſome change; but I had no particular expectation concerning the nature of that change. I had, however, been ſo often favoured with valuable reſults, from merely putting things into new ſituations, that I was encouraged to make the experiment; but I found an unexpected difficulty in getting a cock that would be air-tight and ſteam-tight, under ſo great a preſſure as I wiſhed to apply.

I was mentioning theſe ideas to Mr. WATT, in whoſe neighbourhood I have the happineſs to be ſituated, when he mentioned a ſimilar idea of his, *viz.* that of the poſſibility of the converſion of water, or ſteam, into permanent air; ſaying, that ſome appearances in the working of his fire-engine had led him to expect this. He thought that if ſteam could be made red-hot, ſo that all its latent heat ſhould be converted into ſenſible heat, either this or ſome other change would probably take place in its conſtitution. The idea was new to me, and led me to attend more particularly to my former projects of a ſimilar nature, and I began with lime-ſtone, wiſhing to try the effect of giving a red-heat to lime, in which water only ſhould be previouſly combined, thinking it might poſſibly have the ſame effect with making the water itſelf red-hot.

Accordingly, I took a quantity of well calcined lime, and mixing with it a little water, out of which all air had been carefully boiled, I expoſed

I expoſed it gradually to a ſtrong heat in an earthen retort, ſuch as I had been uſually ſupplied with by Mr. WEDGWOOD (who is as much diſtinguiſhed by his love and generous encouragement of ſcience, as he is by his improvements in his own curious art) not imagining that it could make any difference whether the lime, ſo prepared, ſhould receive its heat in an earthen retort, or in a veſſel of iron or glaſs. Proceeding, however, in this manner, I found that nothing came over in the form of *ſteam*, but that there was a great quantity of *air*, ſeveral hundred times more than the bulk of the water, and at that time there was in it a conſiderable proportion of fixed air, which I imagined might either be that which had not been ſufficiently expelled before, or might be compoſed of ſome phlogiſtic matter contained in the lime, and the purer air that was yielded by the water. For I own I then concluded, that the air which I got (and which, when the fixed air was extracted from it, was ſuch as a candle would juſt burn in) came from the water, eſpecially as in ſome of the proceſſes, the weight of the air I caught was very nearly, if not quite equal to that of the water, and interpoſing a large glaſs balloon between the retort and the recipient for the air, I obſerved that it remained perfectly cool and dry during the whole proceſs; and ſeveral hours afterwards there was not the leaſt moiſture condenſed in it. I alſo received a quantity of another produce of air, made in this manner in mercury, and having viewed it with the greateſt attention, obſerved that, after ſeveral days, it never depoſited the leaſt moiſture.

I then calcined a quantity of natural limeſtone, with this glaſs balloon interpoſed in the ſame manner, and found no water, but only air to come from it, though the ſtone is generally ſuppoſed to contain water. But when I uſed much more than half an ounce of water to the quantity of whiting or lime above-mentioned, I always had ſome water come over, though very little in proportion to the quantity made uſe of.

I did not fail to examine whether there had been any loſs in the weight of the lime, or whiting, in order to determine whether any part of theſe ſolid ſubſtances had entered into the compoſition of the air; but I found much difficulty in weighing them with exactneſs, after ſhaking them out of an earthen retort, into which I could not ſee, and to which part of theſe earthy matters often adhered, ſo that I could not obtain much ſatisfaction even when I broke the retort. Beſides, there was always ſome loſs of the earth in the cloudineſs of the air, whenever the production of it was rapid. In a future proceſs I had abundant proof that the air did not come from any earthy matter with which the water had been combined.

Hitherto I had no idea but that all that was neceſſary to the converſion, as I concluded it to be, of water into air, was to give it a red heat, without which it would not quit the calcareous earth; and I imagined that by this means the matter, or principle, of *heat* was ſo intimately combined with it, as not to be ſeparated from it by cooling, as in the caſe of ſteam. But I, as well as all my friends, was a long time utterly diſconcerted upon finding that when I put the whiting and water into a coated glaſs retort, the water came over in the form of ſteam, and little or no air was produced. The reſult was alſo the ſame when I made the proceſs in a gun-barrel, in a porcelain retort, or even in an earthen retort glazed in the inſide.

That the earth had not loſt its property of doing its part in the buſineſs, I found by putting more water to the ſame whiting which had failed in the glaſs retort, and which had been uſed no leſs than four times before, and then heating it in an earthen retort; when again it gave air only, and no water, the ſame as before. And at this time I obſerved, that part of the air was hardly to be diſtinguiſhed from that of the atmoſphere.

I cannot expreſs my ſurpriſe at my unexpected failure with the glaſs retort; and my ſpeculations on the ſubject were various, but at that time altogether ineffectual. Among other things it occurred to me that, poſſibly, ſome phlogiſton, either contained in the earthen retort, or coming through it (though I could

I could not tell *how*, or on what principle) from the fire, might be neceſſary to water, and all other ſubſtances aſſuming the form of air. But when, with this idea, I put ſpirit of wine, oil, or iron-filings to the lime, I got nothing from theſe mixtures in glaſs retorts beſides ſteam and inflammable air, from the decompoſition of theſe ſubſtances containing phlogiſton.

That there was nothing in the materials of which the earthen retort was made, that neceſſarily produced the air, was evident from my not ſucceeding when I pounded a broken retort, and heated it, mixed with water, in one of glaſs.

Being ſatisfied that the production of air depended very much upon the retort itſelf, I thought of uſing the retort only with water, but without any lime, or earthy ſubſtance; and I found it ſucceed far beyond my expectation. For when I put a ſmall quantity of water into one of theſe retorts, and endeavoured to diſtil it gently, I never failed to procure about an hundred ounce meaſures of air; and this I could do as often as I pleaſed, with the ſame retort, and without its loſing any weight; and the air produced in this manner never had any portion of fixed air in it, and was always but very little inferior to that of the atmoſphere.

In all theſe proceſſes I obſerved, that very little of this air was procured till all the water that could be poured out of the retort was evaporated, for the difference in the produce was very little, whether I expoſed the retort to the fire quite full of water, or with only about an ounce meaſure of water in it, or even after letting it remain full for a ſhort time, and then pouring out all that I could from it; ſo that it was only that water which was entangled, as it were, in the pores of the retort, and which had been in ſome meaſure united to the ſubſtance of it, that had contributed to this production of air.

Theſe retorts (which Mr. WEDGWOOD informs me are made of a mixture of freſh and of burnt Devonſhire pipe clay) are pervious to water, though not to air; ſo that while the air is produced from that water which has entered the pores, the reſt is ſometimes viſibly making its eſcape in the form of a copious ſmoke on the outſide. It was evidently impoſſible, however, and contrary to all the laws of hydroſtatics, that air ſhould enter by the ſame pores by which the water or ſteam was eſcaping, and at the ſame time that its endeavour to force its way out of the retort was ſuch that it overcame a conſiderable reſiſtance from the column of water, at the mouth of my recipients. Air might have *eſcaped* through any unobſerved pores in the retort, but none could have *entered* that way: and if there was the leaſt ſenſible crack in any part of the retort, I was never able to collect any air at all*.

But the following experiments may, perhaps, ſhew that it is ſufficient for the production of air that ſteam come into contact with clay ſufficiently heated. Between a copper ſtill and the glaſs tube communicating with my recipient for air, I introduced the ſtem of a tobacco-pipe; and by means of a ſmall furnace, I kept about three inches of the middle part of it moderately red-hot. In this ſtate, making the water boil, I uniformly received air, though mixed with ſteam, at the rate of five ounce meaſures in twelve minutes for more than an hour; but when I let the pipe cool, nothing but ſteam was delivered by it without any air at all. There was no fixed air in this produce, and it was all ſuch as a candle would hardly have burned it. It might, I thought, have been better, and alſo more in quantity, if I had not uſed the ſtem of a foul pipe. But when I uſed a clean pipe in the ſame manner, I did not find the air much, if at all, improved. Suſpecting this to ariſe from the near contact of the fuel, I incloſed the tobacco-pipe in an earthen tube, and then I had air as good as I had generally got in the earthen retort, and not much worſe than that of the atmoſphere.

---

* It will appear, however, that the air muſt have entered by the pores, but by means of a power very different from that of *preſſure*, and able to counteract it.

Another circumſtance I obſerved was, that if the outſide of the veſſel which contained the water or ſteam, through which it paſſed, when the requiſite heat was applied to it, was not dry, or perhaps ſurrounded with good air (for in thoſe circumſtances the following experiment differs from the preceding ones) the experiment did not ſucceed.

When I put the ball of an earthen retort, filled with moiſt clay, into an iron digeſter, and applied heat to it, I got only a very little fixed air (which was probably compoſed of a ſmall quantity of air beginning to be produced from the materials) and inflammable air from the veſſel. All that came over beſides was ſteam, and at laſt inflammable air from the veſſel itſelf.

Being now able to procure air by means of water, in this moſt ſimple method, *viz.* by water only in the earthen retort, I had an opportunity of aſcertaining, with great eaſe and exactneſs, ſeveral circumſtances relating to the proceſs, and of obviating, as I thought, ſome objections to the concluſion that I had drawn from it. Among other things, I fully ſatisfied myſelf that the *earth of the retort* contributed nothing at all to this production of air, but the *water only*: for having uſed the ſame retort till I had got from it nearly an ounce weight of air, or 800 ounce meaſures, I found that it had not loſt ſo much as a ſingle grain in weight. After the firſt proceſs it weighed juſt three grains more than it did at firſt, and it continued to weigh the ſame till after the laſt proceſs. This ſmall addition of weight might eaſily have come from a little of the water having been imbibed by the neck of the retort, where the heat of the fire could not reach it. When all the proceſſes were over, I kept the whole retort in a red heat for ſeveral hours, and then found that, beſides loſing thoſe three grains, it weighed eight grains leſs than it did at firſt.

Before this I had found, that the calcined whiting which I had uſed in the firſt experiment could not, as ſome had ſuppoſed, attract from the atmoſphere, any conſiderable part of the air which I got from it, after combining water with it: for two ounces of the whiting (which was the quantity that I generally made uſe of) did not attract more than eight grains of any thing when it was expoſed a whole day in an open diſh, though it had loſt more than half its weight in calcination.

It has been imagined by ſome, that the air which I got in theſe earthen retorts was that which had been attracted from the atmoſphere by the inſide ſurface of them. But, beſides that no air could ever be produced without water, to obviate this objection more particularly, when one of theſe retorts was giving its laſt portion of air, I immerſed the mouth of it in a baſon of water; and letting it cool in that ſituation, filled it again without admitting any acceſs of air to the inſide; and yet, on repeating the proceſs with it, the air was produced juſt as freely as before. This operation I repeated ſeveral times. If it be ſaid, that the outſide of the retort attracted the air, ſtill the inſide, being compoſed of the ſame materials, muſt have attracted air alſo; and it would have appeared by the aſcent of the water from the baſon, the retort being ſufficiently impervious to air.

By ſome it was imagined, that either the air itſelf that I procured, or at leaſt the power of the retort to contribute to the production of it, was owing to ſomething that was tranſmitted from the burning coals, but which could not paſs through glaſs or metals. To determine this, I took an earthen tube, of the ſame compoſition with the retort, and putting a little water in it, placed it, ſurrounded with ſand, in a glaſs veſſel, and this again, ſurrounded alſo with ſand, in an iron one; and yet the heat tranſmitted through all theſe ſubſtances, enabled the earthen tube to give air, in the ſame proportion, and of the ſame quality, as it would have done if it had been expoſed to the naked fire.

Having now procured air, by means of water, in a very ſimple, and as I thought, an unexceptionable manner, I wiſhed to make it in greater quantities, in proportion to the water employed; and for this purpoſe I firſt thought of increaſing the ſize, or the thickneſs of the porous retorts: but I thought it might anſwer as well if I put into the retort, in powder, the materials of which they were made, or other ſubſtances of the ſame kind.

Accordingly, by mixing ground flint and clay in various proportions, I preſently increaſed the quantity of air much beyond my expectation. In the firſt trials, in which I had much flint and a little clay, I never failed to get 200 ounce meaſures of air from one of water. Then, uſing more clay and leſs flint, I had ſtill more air; and at laſt, leaving out the flint altogether, and uſing clay only, I never failed to get much more than 400, and generally between 500 and 600 ounce meaſures of air from one of water, which was about three-fourths of the weight of the water; and in one particular proceſs I procured very little leſs than nine-tenths of the weight of the water in air, and this air was never much leſs pure than that of the atmoſphere. Sometimes it could not be diſtinguiſhed from it at all by the teſt of nitrous air; and once or twice I thought it even purer than that of the atmoſphere.

I muſt here obſerve, that I found it not convenient to put ſo much water to any quantity of clay as would make it cohere in one maſs, but only ſo much as that it ſhould remain in the form of powder. By this means it might eaſily be poured out of the retort when the experiment was over.

The weight of the water expended in this production of air I aſcertained, in the moſt unexceptionable manner, by weighing the retort, with all its contents, before and after the proceſs. I ſhall explain this by the reſult of two of the proceſſes. In one of them, the retort and moiſtened clay together loſt in weight 588 grains, after yielding 741 ounce meaſures of air, which (in the proportion of ſix grains to one ounce meaſure) would have weighed 444 grains, and conſequently, about three-fourths of the weight of the water.

In the other proceſs, the loſs of weight was 358 grains, after yielding 556 ounce meaſures of air, which would have weighed 333 grains. The proportion, therefore, between the weight of the air and that of the water was 111 to 116, or nearly nine to ten.

I alſo found now, that ſo much heat as I had hitherto applied was neither neceſſary nor uſeful. In the laſt-mentioned proceſs the retort was conſtantly ſuſpended about ſix inches above a moderate charcoal fire; at another time more than twelve or fifteen inches above it, where a FAHRENHEIT'S thermometer did not ſhew more than 210°. With this moderate heat I got 465 ounce meaſures in the courſe of about twelve hours. When the retort was ſuſpended within ſix inches of the fire, the air was generally produced at the rate of 30 ounce meaſures in five minutes. But a thermometer, the bulb of which was immerſed in the clay, was ſtill only at the heat of boiling water.

In all theſe proceſſes, however, there was evidently ſome loſs of water; for, excepting the firſt experiment with the lime, I never got the whole weight of the water in air; and it might be ſaid that I only expelled the air before contained in the water, though from theſe experiments it appeared to contain much more air than it had been thought capable of containing. To obviate this objection, I contrived to catch all the water that eſcaped through the pores of the retort in the following manner.

Having put the moiſtened clay into an earthen tube, to which I had fitted a cock and a long glaſs tube (by means of which I could collect all the air that came from it) I put this within an iron tube, which was cloſed at the end next the fire, but open at the other end, and ſo long that I could eaſily keep this open, and quite cool, while the other was in the fire; conſequently, whatever water eſcaped through the pores of the earthen tube, it would be condenſed in the cool part of the iron one. This water I carefully collected, and always found that the weight of it, together with that of the air produced in the experiment, was nearly that of the original weight of the water, eſtimated by the loſs of weight in the earthen tube and its contents. I alſo found, that the water ſo collected ſerved for the production of more air, juſt as well as any other water whatever, ſo that there had been no decompoſition of the water in the caſe.

In the laſt proceſs that I went through of this kind, the loſs of weight in the earthen tube, or rather of the water contained in it, was 392 grains; the air collected was 173 ounce meaſures, which would have weighed 89 grains, and the water which eſcaped through the pores of the earthen tube, and which I collected, was nearly 195 grains; ſo that the air and this water together weighed 302 grains, or ten grains more than the original water. But as I eſtimated the weight of the water only by the ſpace which it occupied in a cylindrical glaſs tube, divided according to ounces and parts of ounces of water, it was not eaſy to avoid an error of a few grains. At other times there was an error of a ſmall magnitude on the other ſide. But it will appear hereafter, that more ſteam muſt have eſcaped inviſibly at the open mouth of the iron tube than I was aware of.

That nothing could enter by the pores of the retort at the ſame time that the water was making its eſcape out of them, I thought I aſcertained pretty ſatisfactorily by immerſing the bulb of it in mercury, contained in an iron veſſel. In theſe circumſtances I obtained air as uſual, only the produce was not ſo rapid. In this way, however, I procured above an hundred ounce meaſures of air from moiſtened clay; and I diſcontinued the proceſs without perceiving any termination of it. But the moment the retort was raiſed out of the mercury, it gave air three times as faſt as it had done before. The quality of the air was the ſame in both caſes, *viz.* a little worſe than that of the atmoſphere.

I even collected thirty ounce meaſures of air when the bulb of the ſame retort was immerſed in hot linſeed oil; but the production of air gradually ceaſed, and the next day I found the retort almoſt full of the oil, which had ſoaked through it. Diſtilling this oil, I got 300 ounce meaſures of air wholly inflammable, except a very few ounce meaſures at the laſt, which were only phlogiſticated.

Still hearing of many objections to the converſion of water into air, I now gave particular attention to an experiment of Mr. Cavendish's, concerning the re-converſion of air into water, by decompoſing it in conjunction with inflammable air. And in the firſt place, in order to be ſure that the water I might find in the air was really a conſtituent part of it, and not what it might have imbibed after its formation, I made a quantity of both dephlogiſticated and inflammable air, in ſuch a manner as that neither of them ſhould ever come into contact with water, receiving them as they were produced in mercury; the former from nitre, and in the middle of the proceſs (long after the water of cryſtallization was come over) and the latter from perfectly-made charcoal. The two kinds of air thus produced I decompoſed by firing them together by the electric exploſion, and found a manifeſt depoſition of water, and to appearance in the ſame quantity as if both the kinds of air had been previouſly confined by water.

In order to judge more accurately of the quantity of water ſo depoſited, and to compare it with the weight of the air decompoſed, I carefully weighed a piece of filtering paper, and then having wiped with it all the inſide of the glaſs veſſel in which the air had been decompoſed, weighed it again, and I always found, as nearly as I could judge, the weight of the decompoſed air in the moiſture acquired by the paper.

As there is a ſource of deception in this experiment, in the ſmall globules of mercury, which are apt to adhere to the inſide of the glaſs veſſel, and to be taken up by the paper with which it is wiped, I ſometimes weighed the paper with the moiſture and the mercury adhering to it; and then expoſing it in a warm place, where the water would evaporate, but not the mercury, weighed it again, and ſtill found, as nearly as I could pretend to weigh ſo ſmall a matter, a loſs of weight equal to that of the air. I wiſhed, however, to have had a nicer balance for the purpoſe: the reſult was ſuch as to afford a ſtrong preſumption that the air was re-converted into water, and therefore that the origin of it had been water.

Another preſumption in favour of the generation of our atmoſphere from water was, that the purity of the air that I produced from it is ſo very nearly the ſame with that of the atmoſphere. And the degree of heat requiſite to produce it is no greater than may be given by the rays of the ſun in certain circumſtances. Subterraneous fires, however, would be abundantly ſufficient for the purpoſe, as it appears to be ſufficient for the converſion of water into reſpirable air, that it come into contact with clay, and perhaps many other earthy ſubſtances in the form of vapour. I muſt, however, obſerve, that when I threw the focus of a burning lens upon a quantity of moiſt clay, either *in vacuo*, or in common air, I got no air from it.

I made this experiment both with the clay expoſed in an open diſh, and alſo confined in a ſhort earthen tube. Had I then proceeded to repeat this laſt proceſs with a communication between the inſide of the earthen tube and the external air (as I then propoſed to do, but was prevented) I ſhould much ſooner have diſcovered what I did afterwards, *viz.* that there was no real converſion of water into air in this proceſs. In favour of which, however, it may not be amiſs to obſerve, that the great difficulty Mr. De Luc and others have found in expelling all air from water, is beſt accounted for on the ſuppoſition of the generation of air from water, though in other circumſtances than thoſe that I have obſerved. I have the pleaſure to add, that Mr. De Luc himſelf concurs with me in this opinion.

The difficulty that ſtrikes many perſons moſt forcibly, is the want of analogy between the converſion of water into air with any other known facts in philoſophy or in nature. But admitting that this converſion is effected by the intimate union of what is called the *principle of heat* with the water, it appears to me to be ſufficiently analogous to other changes, or rather combinations, of ſubſtances. Is not the acid of nitre, and alſo that of vitriol, a thing as unlike to air as water is, their properties being as remarkably different? And yet it is demonſtrable, that the acid of nitre is convertible into the pureſt reſpirable air, and probably by the union of the ſame principle of heat*.

---

* It will appear by ſubſequent experiments, that there is no acid in dephlogiſticated air, this being only one of the component parts of *water*.

It is true, that ſteam is a thing very different from air, and I find that it is not able to decompoſe nitrous air; but then, though it has acquired ſenſible heat, it has got no latent heat ſo intimately combined with it as it is with air; and for the ſame reaſon, perhaps, the vapour of nitrous acid is not dephlogiſticated air.

By the ſame proceſs by which reſpirable air ſeems to be made from water, inflammable air may certainly be made from liquid ſubſtances containing phlogiſton. Making ſpirit of wine to boil in a glaſs retort, I made the vapour paſs through the ſtem of a hot tobacco-pipe, and found that it was all converted into inflammable air, and it was of that kind which burns with a lambent white flame. But when I let the pipe cool, no air was produced, but only vapour, which was inſtantly condenſed in the water.

Being now maſter of a new and eaſy proceſs, I was willing to extend it to other liquid ſubſtances; and I preſently found, as I then imagined, that, by this means, I could give a permanent aërial form to any liquid ſubſtance that had been previouſly thrown into the form of vapour.

When I made the vapour of ſpirit of nitre, heated in a glaſs retort, paſs through the ſtem of the hot tobacco-pipe, I got as pure dephlogiſticated air as ever I have procured from nitre; though the cork, by which the retort was connected with the pipe, was diſſolved, and muſt have contributed to contaminate it, and give it a ſlight mixture of fixed air.

With oil of vitriol I got air conſiderably phlogiſticated, ſo that a candle would not have burned in it; but this I alſo attribute to the cork, which was diſſolved in the proceſs. The reſult was nearly the ſame when I uſed water impregnated with vitriolic acid air, though the cork was not diſſolved. But this acid is known to contain much phlogiſton.

Spirit of ſalt gave air no purer than the beſt atmoſpherical air. But as by this proceſs I never got air ſo pure as this from water only, I concluded, that even this acid, as well as the nitrous and vitriolic, is capable of being changed into dephlogiſticated air.

When I uſed water impregnated with fixed air, this air was expelled by the heat, and came over without any change that I could perceive, except that the reſiduum was larger, from the water that came along with it. The air I got afterwards was only that from the water, and of the ſame quality as if it had not been impregnated with fixed air.

Water impregnated with alkaline air gave neither fixed nor inflammable air, which I had rather expected, but only air conſiderably phlogiſticated; though ſome of it was ſo pure that a candle would have burned in it.

N. B. In all theſe experiments with the tobacco-pipe, all the air was remarkably turbid, like milk, and even the common air in the retort before the proceſs properly began.

In this ſtate of the experiments I think I may venture to ſay, that no perſon could have ſeen them without concluding that there was a real converſion of water into air, there being no known principle or fact in philoſophy, that could have led any perſon to ſuſpect a fallacy in the caſe. In this, therefore, I muſt have acquieſced, as indeed did all my acquaintance, even thoſe who had been the moſt incredulous on the ſubject, after they had themſelves ſeen the experiments. But I was led to the farther proſecution of this buſineſs, in conſequence of having obſerved that the *purity* of the air which I procured depended upon the ſtate of that which was immediately contiguous to the earthen retort, or tube, in which I ſuppoſed the converſion to have been made; and that ſome communication with the atmoſphere was neceſſary to the production of any air, as in the experiment with the digeſter, and thoſe with the clay and the burning lens. And ſince pure external air was neceſſary in order to procure good air, it was concluded by ſeveral of my friends, and eſpecially Mr. WATT, that the operation of the earthen retort was, to tranſmit phlogiſton from the water contained in the clay to the external air; and that the water, thus dephlogiſticated, was capable of being converted into reſpirable air by the intimate union of the principle of heat.

In order to aſcertain what the influence of the external air in this caſe really was, I incloſed an earthen retort filled with moiſtened clay, in a large glaſs receiver, open at both ends, through the upper orifice of which (being narrow) I thruſt the neck of the retort, luting it ſo as to be perfectly air-tight; and placing the receiver in a baſon of water, by which the air within was cut off from all communication with the external air, I fitted to the mouth of the retort a glaſs tube, through which I could receive whatever was produced in the proceſs*. In this ſituation I heated the retort, by means of Mr. Parker's excellent burning lens, when air was received through the tube communicating with the inſide of the retort as uſual; but at the ſame time the water roſe within the receiver. This effect might be owing to a phlogiſtication of the air within the receiver; but it was ſoon diminiſhed far beyond the utmoſt limit of that proceſs, ſo that very little of it remained; and examining this air, I found it to be nearly the ſame with that of the atmoſphere, as alſo was that which came from the retort.

---

* Fig. I. exhibits a view of the diſpoſition of the apparatus in this experiment, and in many others of the ſame nature.

This experiment made it probable, that the air on the outſide of the receiver had actually paſſed through it; and yet it was contrary to all the known principles of hydroſtatics, and even any thing hitherto known in chemiſtry, that air ſhould be tranſmitted through a veſſel of this kind, and in a direction contrary to that in which it would have been forced by the preſſure of the atmoſphere; while the water, with which the clay was moiſtened, went the other way. For had the retort been pervious to air, as the inſide had a free communication with the atmoſphere, the water could not have riſen within the receiver. This, however, appeared to be the caſe, by the following deciſive experiments.

Having filled the earthen retort with the moiſtened clay as before, I made the inſide of the receiver perfectly dry, and placed it in a baſon of mercury; when, upon heating the retort as before, the receiver was all covered with dew, which collecting into drops trickled down the inſide of the receiver, and remained upon the mercury, which roſe within the receiver, while air was received from the retort as uſual. I had no doubt, therefore, but that all the water within the retort would have got through into the receiver. Spirit of wine, or ſomething that had the ſmell of it, was tranſmitted from the clay through the retort in the ſame manner.

I then filled the receiver with inflammable air, and upon heating the retort, it was all drawn through it, and delivered as ſtrongly inflammable as ever by the tube communicating with the inſide of it; while the water roſe within the receiver, and even covered the retort, which was fixed at the very top of it, ſo that hardly any of the inflammable air remained within it. In like manner nitrous air paſſed through the retort unchanged.

From theſe experiments it ſeemed impoſſible not to infer, that the clay of the earthen retort, being thus heated, deſtroys for a time the aërial form of whatever air is expoſed to the outſide of it; which aërial form it recovers after it has been tranſmitted in combination from one part of the clay to another, till it has reached the inſide of the retort, while the water is drawn through it in a contrary direction*.

Had this hypotheſis been propoſed *a priori*, it would, I doubt not, have been thought more extraordinary than the converſion of water into air. I propoſe to make many other experiments in the proſecution of theſe; but till I have an opportunity of doing this, I ſhall not trouble the Society with any conjectures that have occurred to me on the ſubject.

---

* Notwithſtanding this, reaſons will preſently be given, which ſhew that this is no juſt concluſion, and that the proceſs muſt have been carried on by means of *pores*.

The great difficulty with reſpect to the experiment with the lens is, that the water ſhould paſs through the retort one way, and the air the other, and yet that the air ſhould not be able to paſs without the water. It is alſo not a little extraordinary, that the weight of the air and that of the water ſhould be ſo nearly equal.

In the laſt place I muſt obſerve, that there is nothing in this experiment that contradicts the idea of the converſion of water into air, though it does not prove it: for ſtill the experiment with the tobacco-pipe, in which the ſteam is made red-hot (whereas in that with the lens it is only of a boiling heat) cannot be explained ſo well on any other hypotheſis any more than Mr. CAVENDISH's experiment on finding water on the decompoſition of air.

I cannot conclude this account without acknowledging my obligation to Mr. PARKER for the uſe of his incomparable lens, and his obligingly aſſiſting me in the management of it. Indeed, without this very inſtrument, or one of greater power than my own, I do not know that the laſt-mentioned experiments could have been made at all; certainly not to ſo much ſatisfaction*.

Experiments on the ſeeming Converſion of Water into Air continued.

When I wrote the preceding article, it was my opinion, that the air which was tranſmitted through the earthen retort, had loſt for a moment its *form of air*, and was properly incorporated with the *earth*, as fixed air is in limeſtone, and that it had reſumed its aërial form in the inſide of the retort; becauſe theſe veſſels had been air-tight in all my other experiments. But though theſe earthen retorts be ſufficiently air-tight for many purpoſes, and we cannot blow through them when they are plunged in water, yet unleſs they be *glazed*, they are not ſo air-tight, but that, with an air-pump, air may be drawn through them. Conſequently, any power acting with the force of an air-pump (which is only equal to the preſſure of the atmoſphere) may overcome the difficulty of air paſſing through the ſmall pores of ſuch retorts; and there are many powers in chemiſtry much greater than this.

---

* By means of ſmall earthen veſſels, of an inch diameter in the bulb, ſuch as is repreſented in fig. I. the experiment is made with ſufficient advantage, with a lens of 12 inches in diameter.

At preſent it is my opinion, that the agent in this caſe is that principle which we call *attraction of coheſion*, or that power by which water is raiſed in capillary tubes. But in what manner it acts in this caſe, I am far from being able to explain. Much leſs can I imagine how *air* ſhould paſs one way, and *vapour* the other, in the ſame pores, and how the tranſmiſſion of the one ſhould be neceſſary to the tranſmiſſion of the other. I am ſatisfied, however, that it is by means of ſuch pores as air may be forced through, that this curious proceſs is performed, becauſe the experiment never ſucceeds but in ſuch veſſels as, by the air-pump at leaſt, appear to be *porous*, though in all ſuch. But the ſmallneſs of the pores ſeems to be rather an advantage in this experiment; and the ſmalleſt capillary tubes alſo are known to act with the greateſt force.

A veſſel ſcooped out of *chalk* anſwered as well as an earthen retort; ſo alſo did one of *free-ſtone*, firſt about a quarter of an inch in thickneſs, and then very near an inch, which I got made for the purpoſe; and ſo alſo did a veſſel of *white marble* of the cloſeſt texture, though it is ſufficiently air-tight for moſt purpoſes, and nothing but the air-pump could have convinced me that it had any pores.

In order to meaſure the force of this attraction, I placed the glaſs veſſel through which I tranſmitted the rays of the ſun (and which contained the air that was to be drawn through the retort) in a baſon of mercury, and found that while the vapour of water was tranſmitted from moiſtened clay in the inſide of the retort, into the inſide of the glaſs veſſel (and conſequently the air paſſed the contrary way, ſo as to enable the atmoſphere to preſs the mercury into the inſide of the glaſs veſſel) it roſe $3\frac{1}{2}$ inches above the level on the outſide of the glaſs. It might probably have riſen ſomething higher, if all the water in the clay had not then been expended. This was a veſſel made on purpoſe, of pipe-clay only, and very compact. When I uſed more porous earthen veſſels, I was never able to raiſe the mercury ſo high. For when the pores are large, the mechanical action occaſioned by the weight of ſuch a column of mercury counteracts the chemical one, and prevents the tranſmiſſion of the air or ſteam.

Having ſufficiently aſcertained what kind of *veſſels* would anſwer for this experiment, I then tried what kind of *fluid ſubſtances* would, by their tranſmiſſion in one direction, promote the paſſing of the air in the other; and I found that *oils*, as well as *ſpirit of wine*, anſwered this purpoſe perfectly well; and ſo alſo did *mercury*, but not any other metal made fluid by heat.

The mercury paſſed through the retort in the form of a ſubtle *black powder*; and on this occaſion I obſerved that the veſſel of pipe-clay with which the experiment was made, was ſo far air-tight, that when I took it to the air-pump, nearly full of mercury, quite hot from the experiment in the ſun, and placed it upon the plate of the air-pump, I was not able to force any mercury through it. Probably this black powder was much more attenuated than mercury in its natural fluid ſtate can be. There is alſo a degree of repulſion between white mercury and all glaſs or earthen veſſels. This repulſion mechanical preſſure might not be able to overcome, though this chemical action could do it. It remains, however, to be conſidered, how the mercury in this proceſs could be changed from its natural fluid ſtate into this black powder.

While I was making theſe experiments with mercury, I took the opportunity of trying one of the kinds of air that cannot be confined by water; and having filled the glaſs veſſel with *alkaline* air, and having mercury in the veſſel of clay; I ſoon found, by the ſmell, that the alkaline air was paſſing one way, while the mercury went the other, juſt as readily, I doubt not, as inflammable or nitrous air had gone one way, while water had gone the other.

Of the *metals* in a ſtate of fluidity, I tried *lead*, *tin*, *zinc*, and *biſmuth*, melting them in porous earthen retorts; but neither did any metal paſs one way, or air the other.

I made ſome attempts to perform this experiment with veſſels which had *viſible pores*, eſpecially with fine capillary tubes of glaſs; having the ſtoppers of the glaſs tubes made with fine perforations of this kind, but hitherto without ſucceſs; and yet I do not abſolutely deſpair of ſucceeding in an experiment of this kind, and thereby ſeeing a little farther into this curious proceſs.

SECTION III.

*Experiments and Obſervations relating to Air and Water**.

EVER ſince the diſcovery of the diminution of reſpirable air in thoſe proceſſes which are generally called *phlogiſtic*, it has been a great object with philoſophers to find what becomes of the air which diſappears in them. Among others, I have made and publiſhed a variety of experiments with that view; but though by this means ſome farther progreſs was made in the philoſophy of air, and conſequently our knowledge of the principles, or conſtituent parts, of natural ſubſtances was extended, I did not by any means ſucceed to my ſatisfaction with reſpect to the immediate object of my reſearches. Others, however, were more ſucceſsful, and their ſucceſs has at length enabled me to reſume my experiments with more advantage; by which means I have been led both to confirm their concluſions, and, by diverſifying the experiments, to throw conſiderable light upon various other chemical proceſſes. The reſult of theſe obſervations I ſhall lay before the Society, with as much brevity and diſtinctneſs as I can.

---

* This ſection is copied from the Philoſophical Tranſactions, vol. lxxv. p. 279.

In the experiments of which I ſhall now give an account, I was principally guided by a view to the opinions which have lately been advanced by Mr. CAVENDISH, Mr. WATT, and M. LAVOISIER. Mr. CAVENDISH was of opinion, than when *air* is decompoſed, *water* only is produced; and Mr. WATT concluded from ſome experiments, of which I gave an account to the Society, and alſo from ſome obſervations of his own, that water conſiſts of dephlogiſticated and inflammable air, in which Mr. CAVENDISH and M. LAVOISIER concur with him; but Mr. LAVOISIER is well known to maintain, that there is no ſuch thing as what has been called *phlogiſton*, affirming inflammable air to be nothing elſe but one of the elements or conſtituent parts of water. In the following experiments I alſo had a particular view to a concluſion which I had drawn from thoſe experiments, of which an account is given in my laſt communications to the Royal Society, *viz.* that inflammable air is pure phlogiſton in the form of air, at leaſt with the element of *heat*; and that fixed air conſiſts of dephlogiſticated and inflammable air; both which doctrines had been firſt advanced by Mr. KIRWAN, before I had made the experiments which I then thought clearly proved them.

Such were the hypotheſes to which I had a view, when I began the following courſe of experiments, which I hope will be an admonition to myſelf, as well as to others, to adhere as rigorouſly as poſſible to *actual obſervations*, and to be extremely careful not to overlook any circumſtance that may poſſibly contribute to any particular reſult. I ſhall have occaſion to notice my own miſtakes with reſpect to *concluſions*, though all the *facts* were ſtrictly as I have repreſented them. But whilſt philoſophers are faithful narrators of what they obſerve, no perſon can juſtly complain of being miſled by them; for to *reaſon* from the facts with which they are ſupplied, is no more the province of the perſon who diſcovers them, than of him to whom they are diſcovered.

One of the moſt ſimple of all phlogiſtic proceſſes, is that in which metals are ignited in dephlogiſticated air. I therefore began with this, with a view to aſcertain whether any *water* be produced when the air is made to diſappear in it. Accordingly, into a glaſs veſſel containing ſeven ounce meaſures of pretty pure dephlogiſticated air, I introduced a quantity of iron turnings (which is iron in ſmall thin pieces, exceedingly convenient for theſe and many other experiments) having previouſly made them, together with the veſſel, the air, and the mercury by which it was confined as dry as I poſſibly could. Alſo, to prevent the air from imbibing any moiſture, I received it immediately in the veſſel in which the experiment was made, from the proceſs of procuring it from red precipitate; ſo that it had never been in contact with any water.

I then fired the iron, by means of a burning lens, and preſently reduced the 7 ounce meaſures of air to .65 of a meaſure; but I found no more water after this proceſs than I imagined it had not been poſſible for me to exclude, as it bore no proportion to the air which had diſappeared. Examining the reſiduum of the air, I found one-fifth of it to be fixed air, and when I tried the purity of that which remained by the teſt of nitrous air, it did not appear that any phlogiſticated air had been produced in the proceſs; for though it was more impure than I ſuppoſe the air with which I began the experiment muſt have been, it was not more ſo than the phlogiſticated air of the 7 ounce meaſures, which had not been affected by the proceſs, and which muſt have been contained in the reſiduum, would neceſſarily make it. In this caſe one meaſure of this reſiduum and two of nitrous air occupied the ſpace of .32 of a meaſure.

In another experiment of this kind, ten ounce meaſures of dephologiſticated air were reduced to .8 of a meaſure, and by waſhing in lime water to .38. In another experiment, in which 7 ½ ounce meaſures of dephlogiſticated air were reduced to half an ounce meaſure, of which one-fifth was fixed air, the reſiduum was quite as pure as the air with which I began the experiment, the teſt with nitrous air, in the proportions above-mentioned, giving .4 in both caſes. To what circumſtance the difference might be owing I cannot tell.

In theſe experiments the fixed air muſt, I preſume, have been formed by the union of the phlogiſton from the iron and the dephlogiſticated air in which it was ignited; but the quantity of it was very ſmall in proportion to the air which had diſappeared, and at that time I had no ſuſpicion that the iron, which had been melted, and gathered into round balls, could have imbibed it; a melting heat having been ſufficient, as I had imagined, to expel every thing that was capable of aſſuming the form of air from any ſubſtance whatever. I was therefore entirely at a loſs about what muſt have become of the air.

Senſible, however, that ſuch a quantity of air muſt have been imbibed by *ſomething*, to which it muſt have given a very perceivable addition of weight, and ſeeing nothing elſe that could have imbibed it, it occurred to me to weigh the calx into which the iron had been reduced; and I preſently found, that the dephlogiſticated air had actually been imbibed by the melted iron, in the ſame manner as inflammable air, in my former experiments, had been imbibed by the melted calces of metals, however improbable ſuch an abſorption might have appeared to me *a priori*. In the firſt inſtance, about twelve ounce meaſures of dephlogiſticated air had diſappeared, and the iron had gained ſix grains in weight. Repeating the experiment very frequently, I always found, that other quantities of iron, treated in the ſame manner, gained ſimilar additions of weight, which was always very nearly that of the air which had diſappeared.

This calx of iron, I then concluded, was by no means what I had before taken it to be, *viz.* a *pure calx*, or *ſlag*, but either the calx, or the iron itſelf, ſaturated with pure air. This calciform ſubſtance I found, by various experiments, to be the ſame thing with the *ſcales* that fly from iron when it is made red-hot, or the ſubſtance into which it runs in a very intenſe heat, in an open fire*.

Concluding from the preceding experiment, that iron ſufficiently heated, was capable of ſaturating itſelf with pure air, extracted from the maſs of the atmoſphere, I then proceeded to melt it with the heat of a burning lens in the open air; and I preſently found, that perfect iron was eaſily fuſed in this way, and continued in this fuſion a certain time, exhibiting the appearance of *boiling* or *throwing out* air, whereas it was on the contrary *imbibing* air; and when it was ſaturated the fuſion ceaſed, and the heat of my lens could not make any farther impreſſion upon it. When this was the caſe, I always found that it had gained weight in the proportion of 7½ to 24, which is very nearly *one-third* of its original weight. The ſame was the effect when I melted *ſteel* in the ſame circumſtances, and alſo every kind of iron on which the experiment could be tried. But I have ſome reaſon to think, that with a greater degree of heat than I could apply, the iron might have been kept in a ſtate of fuſion ſomewhat longer, and by that means have imbibed more air, even more than one-third of its original weight.

---

* It is alſo the ſame thing with *finery cinder*, of which an account will be given hereafter.

There was a peculiar circumſtance attending the melting of *caſt iron* with a burning lens, which made it impoſſible to aſcertain the addition that was made to its weight, and at the ſame time afforded an amuſing ſpectacle; for the moment that any quantity of it was melted, and gathered into a round ball, it began to diſperſe in a thouſand directions, exhibiting the appearance of a moſt beautiful fire-work, ſome of the particles flying to the diſtance of half a yard from the place of fuſion; and the whole was attended with a conſiderable hiſſing noiſe. Some of the largeſt pieces which had been diſperſed in this manner I was able to collect, and having ſubjected them to the heat of the lens, they exhibited the ſame appearance as the larger maſs from which they had been ſcattered.

When I melted this caſt iron in the bottom of a deep glaſs receiver, in order to collect all the particles that were diſperſed, they firmly adhered to the glaſs, melting it ſuperficially, though without making it crack, ſo that it was ſtill impoſſible to collect and weigh the particles. However, I generally found that, notwithſtanding the copious diſperſion, what remained after the experiment rather exceeded than fell ſhort of the original weight of the iron. Sometimes a piece of common iron, and eſpecially ſteel, would make a little hiſſing in the fuſion, and a particle or two would fly off; but this was never conſiderable*.

---

* On being informed of the above-mentioned phænomena, Mr. WATT concluded, that the baſis of the dephlogiſticated air united to the phlogiſton of the iron, and formed

Having now procured what I thought to be a new calx of iron, or a calx ſaturated with pure air, I endeavoured to revive it by making it imbibe inflammable air, in the ſame manner that I had before made iron, and various other metals, by melting them in a veſſel containing inflammable air. In this I ſucceeded; but in the courſe of the experiment a new and very unexpected appearance occurred. I took a piece of iron which I had ſaturated with pure air, and putting it into a glaſs veſſel containing inflammable air, confined by water, threw upon it the focus of the lens, and preſently perceived the inflammable air to diſappear, and without thinking of any thing eſcaping from the calx of iron (which had been ſubjected to a greater heat before) I imagined that I ſhould have found the addition of the weight of air in the iron, and the reſult might be an iron different from the

---

formed *water*, which was attracted by, and remained ſo firmly united to the calx of iron, as to reſiſt the effects of heat to ſeparate them.

common ſort. But I found, to my ſurpriſe, that the iron which had exhibited no new appearance in this mode of treatment, had loſt weight, inſtead of gaining any. The piece of iron on which I made this firſt experiment weighed 11½ grains, and 7½ ounce meaſures of inflammable air had diſappeared while the iron had loſt 2½ grains.

Conſidering the quantity of inflammable air that had diſappeared, *viz.* 7½ ounce meaſures, and the dephlogiſticated air which had been expelled from the iron, *viz.* 2½ grains, which is equal to about 4.1 ounce meaſures, I found that they were very nearly in the proper proportion to ſaturate each other, when decompoſed by the electric ſpark, *viz.* two meaſures of inflammable air to one of dephlogiſticated air. I therefore had now no doubt but that the two kinds of air had united, and had formed either *fixed air* or *water*; but which it was I could not tell, having had water in the receiver in which the experiment was made, and having neglected to examine the ſtate of the air that remained, except in a general way, by which

I found, that it was ſtill, to appearance, as inflammable as ever.

With a view to determine whether *fixed air*, or *water*, would be the produce of this mode of combining inflammable and dephlogiſticated air, I repeated the experiment in a veſſel in which the inflammable air was confined by mercury, and both the veſſel and the mercury had been previouſly made as dry as poſſible. I had no ſooner begun to heat the iron, or rather *ſlag*, in theſe circumſtances, than I perceived the air to diminiſh, and at the ſame time the inſide of the veſſel to grow very cloudy, with particles of dew that covered almoſt the whole of it. Theſe particles by degrees gathered into drops, and ran down the ſides of the veſſel in all places, except where it was heated by the ſun-beams; ſo that it then appeared to me very evident, that *water*, with or without fixed air, was the produce of the inflammable air, and the pure air let looſe from the iron in this mode of operation; though I was afterwards taught by Mr. WATT to correct this hypotheſis, and to account for this reſult in a different manner. When I had examined the remaining air, it was as inflammable as ever, without containing any mixture of fixed air at all.

When I collected the water which was produced in this experiment, by means of a piece of filtering paper, carefully introduced to abſorb it, I found it to be, as nearly as poſſible, of the ſame weight with that which had been loſt by the iron: and alſo, in every experiment of this kind, in which I attended to this circumſtance, I found that the quantity of inflammable air which had diſappeared, was about double to that of the dephlogiſticated air ſet looſe from the iron, ſuppoſing that weight to have been reduced into air. Thus at one time I made a piece of this ſlag imbibe 5½ ounce meaſures of inflammable air, while it loſt as much as the weight of about 3 ounce meaſures of dephlogiſticated air, and the water collected weighed 2 grains. Another time a piece of ſlag loſt 1.5 grains, and the water produced was 1.7 grains; but perfect accuracy is not to be expected. I ſhall only mention one more experiment of this kind, in which $6\frac{1}{2}$ ounce meaſures of inflammable air were reduced to .92 of an ounce meaſure, and the iron had loſt 2 grains, equal in weight to 3.3 ounce meaſures of dephlogiſticated air. In all the above-mentioned experiments, the inflammable air was that which is produced by the ſolution of iron in acids.

As before I had finiſhed this courſe of experiments I had ſatisfied myſelf that inflammable air always contains a portion of water, and alſo, that when it has been ſome time confined by water, it imbibes more, ſo as to be increaſed in its ſpecific gravity by that means, I repeated the experiment with inflammable air which had not been confined by water, but which was received in a veſſel of dry mercury from the veſſel in which it was generated; but I preſently perceived that water was produced in this caſe alſo, and to appearance as copiouſly as in in the former experiment. Indeed, the quantity of water produced, which ſo greatly exceeded the weight of all the inflammable air, is ſufficient to prove that it muſt have had ſome other ſource than any conſtituent part of that air, or the whole of it, together with the water contained in it, without taking into conſideration the correſponding loſs of weight in the iron.

I muſt here obſerve, that the iron ſlag which I had treated in this manner, and which had thereby loſt the weight which it had acquired by melting in dephlogiſticated air, became *perfect iron* as at firſt, and was then capable of being melted by the burning lens again; ſo that the ſame piece of iron would ſerve for theſe experiments as long as the operator ſhould chuſe. It was evident, therefore, that if the iron had loſt its phlogiſton in the preceding fuſion, it had acquired it again from the inflammable air which it had abſorbed; and I do not ſee how the experiment can be accounted for in any other way, which neceſſarily implies the reality of phlogiſton as a conſtituent principle in bodies. This, at leaſt, is the moſt natural way of accounting for the appearances.

Having had this ſucceſs with the calx, or ſcales of *iron*, I tried the calx of *copper*, or thoſe ſcales which fly from it when it is made red-hot; and I found water produced in the inflammable air in the ſame manner as when I uſed the ſcales of iron in the ſame circumſtances. I alſo had the ſame reſult when I revived *precipitate per ſe* in inflammable air; but having at that time a very weak winter's ſun, I could not make the experiment with ſo much advantage as I could have wiſhed *.

Iron, I found, acquired this additional weight by melting in an earthen retort, as well as in the open air by the ſun-beams, if it were poſſible for it to attract air, or whatever elſe it is that is the immediate cauſe of its additional weight. Three ounces of common iron filings, expoſed to a ſtrong heat in an earthen retort, gained

---

* It will be ſeen that when I repeated this experiment to more advantage afterwards, I found no more water than might be ſuppoſed to have been contained, as an extraneous ſubſtance, either in the inflammable air, or the red precipitate; which makes a ſtriking contraſt with the experiment in which *iron* is uſed, and is greatly in favour of the doctrine of phlogiſton.

11 dwts. or 264 grains, and yet was very far from having been completely fuſed. Having a glaſs tube communicating with the retort, in order to collect any air that the iron filings might give out, I found that when they were very hot, the water aſcended within the tube; which ſhews that the iron was then in a ſtate of abſorbing, and not of giving out any air.

Seeing ſo much water produced in theſe experiments with inflammable air, I was particularly led to reflect on the relation which they bore to each other, and eſpecially Mr. CAVENDISH's ideas on the ſubject. He had told me that, notwithſtanding the experiments of which I had given an account to the Royal Society, and from which I had concluded that inflammable air was pure phlogiſton, he was perſuaded that *water* was eſſential to the production of it, and even entered into it as a conſtituent principle. At that time I did not perceive the force of the arguments which he ſtated to me, eſpecially as, in the experiments with charcoal, I totally diſperſed any quantity of it with a burning lens *in vacuo*, and thereby filled my receiver with nothing but inflammable air. I had no ſuſpicion that the wet leather on which my receiver ſtood could have any influence in the caſe, while the piece of charcoal was ſubject to the intenſe heat of the lens, and placed ſeveral inches above the leather. I had alſo procured inflammable air from charcoal in a glazed earthen retort two whole days ſucceſſively, in which it had given inflammable air without intermiſſion. Alſo iron filings in a gun-barrel, and a gun-barrel itſelf, had always given inflammable air whenever I tried the experiment.

Theſe circumſtances, however, deceived me, and perhaps would have deceived any other perſon; for I did not know, and could not have believed, the powerful attraction that *charcoal*, or *iron*, appear to have for *water* when they are intenſely hot. They will find, and attract it, in the midſt of the hotteſt fire, and through any pores that may be left open in a retort; and iron filings are ſeldom ſo dry as not to have moiſture enough adhering to them, capable of enabling them to give a conſiderable quantity of inflammable air. But my attention being now fully awake to the ſubject, I preſently found that the circumſtances above-mentioned had actually miſled me; I mean with reſpect to the *concluſion* which I drew from the experiments, and not with reſpect to the experiments themſelves, every one of which, I doubt not, will be found to anſwer, whenever they are tried by perſons of ſufficient ſkill and properly attentive to all the circumſtances.

Being thus apprized of the influence of unperceived moiſture in the production of inflammable air, and willing to aſcertain it to my perfect ſatisfaction, I began with filling a gun-barrel with iron filings in their common ſtate, without taking any particular precaution to dry them, and I found that they gave air as they had been uſed to do, and continued to do ſo many hours: I even got ten ounce meaſures of inflammable air from two ounces of iron filings in a coated glaſs retort. At length, however, the production of inflammable air from the gun-barrel ceaſed; but on putting water into it, the air was produced again, and a few repetitions of the experiment fully ſatisfied me that I had been too precipitate in concluding that inflammable air is pure phlogiſton.

I then repeated the experiment with the charcoal, making the receiver, the ſtand on which I placed the charcoal, and the charcoal itſelf, as dry and as hot as poſſible, and uſing cement inſtead of a wet leather to exclude the air. In theſe circumſtances I was not able, with the advantage of a good ſun, and an excellent burning lens, to decompoſe quite ſo much as two grains of the piece of charcoal, which gave me ten ounce meaſures of inflammable air; and this, I imagine, was effected by means of ſo much moiſture as was depoſited from the air in its ſtate of rarefaction, and before it could be drawn from the receiver. To the production of this kind of inflammable air I was therefore now convinced, that water is as neceſſary as to that from iron.

It was in this ſtate of my experiments that I received an authentic account of thoſe of M. Lavoisier, on tranſmitting water through an hot iron tube, and alſo through a hot copper tube containing charcoal, and thereby procuring large quantities of inflammable air, M. Lavoisier himſelf having been ſo obliging as to ſend me a copy of his Memoir on that ſubject. I had heard an account of the experiments ſome months before; but it was ſo imperfect a one, that I own I paid little attention to them. At this time, however, I was prepared to be ſufficiently ſenſible of their value.

In my laſt communications to the Royal Society, it will be ſeen that I had tranſmitted the vapour of ſeveral fluid ſubſtances through red-hot *earthen tubes*, and thereby procured different kinds of air. M. Lavoisier adopted the ſame proceſs, but uſed an *iron tube*; and by means of that circumſtance made a very valuable diſcovery which had eſcaped me. I had indeed on one occaſion made uſe of an iron tube, and tranſmitted ſteam through it; but not having at that time any view to the production of *air*, I did not collect it at all, contenting myſelf with obſerving that *water*, after being made red-hot, was ſtill water, there being no change in its ſenſible properties. Being now farther inſtructed by the experiment of M. LAVOISIER, I was determined to repeat the proceſs with all the attention I could give to it; but I ſhould not have done this with ſo much advantage, if I had not had the aſſiſtance of Mr. WATT, who always thought that M. LAVOISIER's experiments by no means favoured the concluſion that he drew from them. As to myſelf, I was a long time of opinion that his concluſion was juſt, and that the inflammable air was really furniſhed by the water being decompoſed in the proceſs. But though I continued to be of this opinion for ſome time, the frequent repetition of the experiments, with the light which Mr. WATT's obſervations threw upon them, ſatisfied me at length that the inflammable air came principally from the charcoal, or the iron.

I ſhall firſt relate the reſult of the experiment that was made with *charcoal*, and then thoſe with iron and other ſubſtances, in contact with which (when they were in a ſtate of fuſion, or at leaſt red-hot) I made ſteam, or the vapour of other liquid ſubſtances, to paſs. I ſhall only obſerve that, previous to this, I began to make the experiments with coated glaſs tubes, which I found to anſwer very well during the proceſs, though they never failed to break in cooling. At length I procured a tube of *copper*, on which, as M. LAVOISIER diſcovered, ſteam had no effect; and at laſt I made uſe of earthen tubes, with which Mr. WEDGWOOD, that moſt generous promoter of ſcience, liberally ſupplied me for the purpoſe; and theſe, glazed on the outſide only, I find far preferable to copper. They are, indeed, every thing that I could wiſh for in experiments of this kind; the reaſon of which will appear in my account of another courſe of experiments, which I

I hope to lay before the Society in due time*.

The diſpoſition of the apparatus, with which theſe experiments were made, was as follows: The water was made to boil in a glaſs retort, which communicated with the copper or earthen tube that contained the charcoal or iron, &c. and which, being placed in an horizontal poſition, was ſurrounded with hot coals. The end of this tube oppoſite to the retort communicated with the pipe of a common *worm tub*, ſuch as is generally uſed in diſtillations, by means of which all the ſuperfluous ſteam was condenſed, and collected in a proper receptacle, while the air which had been produced, and had come along with it through the worm-tub, was tranſmitted into a trough of water, where proper veſſels were placed to receive it, and aſcertain the quantity of it; after which I could examine the quality of it at leiſure †.

---

* An account of them is contained in this volume, not being thought of ſufficient importance to lay before the Royal Society.

† The diſpoſition of this apparatus may be ſeen Fig. 2.

In the experiment with *charcoal*, I found unexpected difficulties, and conſiderable variations in the reſult; the proportion between the *charcoal* and *water* expended, and alſo between each of them and the *air* produced, not being ſo nearly the ſame as I imagined they would have been. Alſo the quantity of fixed air that was mixed with the inflammable air varied very much. This laſt circumſtance, however, ſome of my experiments may ſerve to explain. Whenever I had no more water than was ſufficient for the production of the air. there was never any ſenſible quantity of uncombined fixed air mixed with the inflammable air from charcoal. This was particularly the caſe when I produced the air by means of a burning lens in an exhauſted receiver, and alſo in an earthen retort with the application of an intenſe heat. I therefore preſume, that when the ſteam tranſmitted through the hot tube containing the charcoal was very copious, the fixed air in the produce was greater than it would otherwiſe have been. The extremes that I have obſerved in the proportion of the fixed to the inflammable air have been from one-twelfth to one-fifth of the whole. As I generally produced this air, the latter was the uſual proportion; and this was excluſive of the fixed air that was intimately combined with the inflammable air, and which could not be ſeparated from it except by decompoſition with dephlogiſticated air; and this combined fixed air I ſometimes found to be one-third of the whole maſs, though at other times not quite ſo much.

To aſcertain this, I mixed one meaſure of this inflammable air from charcoal (after the uncombined fixed air had been ſeparated from it by lime-water) with one meaſure of dephlogiſticated air, and then fired them by the electric ſpark. After this I always found that the air which remained made lime-water very turbid, and the proportion in which it was now diminiſhed, by waſhing in lime-water, ſhewed the quantity of fixed air that had been combined with the inflammable. That the fixed air is not *generated* in this proceſs, is evident from there being no fixed air found after the exploſion of dephlogiſticated air and inflammable air from iron[*].

Notwithſtanding the above-mentioned variations, the loſs of weight in the charcoal was always much exceeded by the weight of the water expended, which was generally more than double that of the charcoal; and this water was intimately combined with the air; for when I received a portion of it in mercury, no water was ever depoſited from it.

The experiment which, upon the whole, gave me the moſt ſatisfaction, and the particulars of which I ſhall therefore recite, was the following. Expending 94 grains of perfect charcoal (by which I mean charcoal made with a very ſtrong heat, ſo as to expel all fixed air from it) and 240 grains of water, I procured 840 ounce meaſures of air, one-fifth of which was fixed air, and of the inflammable part nearly one-third more appeared to be fixed air by decompoſition.

---

[*] When I wrote this paper, I imagined that the *fixed* air, which was found on the decompoſition of this inflammable air with dephlogiſticated air, had been contained in the inflammable air. But it will appear, in the courſe of this volume, that it muſt have been formed by the union of phlogiſton (or inflammable air) and dephlogiſticated air, made by the exploſion; though it is remarkable that no fixed air is formed when the inflammable air from iron is uſed.

Receiving this kind of air in a variety of experiments, but not in the preceding ones in particular (for then I could not have aſcertained the quantity of it) conſiſting of fixed and inflammable air together, I found ſome variations in its ſpecific gravity, owing, I imagine, to the different proportions of fixed air contained in it; but upon the whole, I think, that the proportion of 14 grains to 40 ounce meaſures is pretty near the truth, when the proportion of fixed air is about one-fifth of the whole. With reſpect to the weight of the inflammable air after the fixed air was ſeparated from it, I found no great difference, and think it may be eſtimated at 8 grains to 30 ounce meaſures.

Upon theſe principles, the whole weight of the 840 ounce meaſures of air will be - - - - - - 294 grains

that of the charcoal will be 94
that of the water - - 240
334 which, conſidering the nature of the experiment, will perhaps be thought to be tolerably near to that of the air.

If the air be analyzed, the 840 ounce meaſures will be found to contain

168 of uncombined fixed air = 151 grains.
and 672 impure inflammable = 179
ſo that the whole 840 will weigh - - 330

It may, however, be ſafely concluded from this experiment, and indeed from every other that I made with charcoal, that there was no more pure inflammable air produced than the charcoal itſelf may be very well ſuppoſed to have ſupplied.

There is, therefore, no reaſon for deſerting the old eſtabliſhed hypotheſis of *phlogiſton* on account of theſe experiments, ſince the fact is by no means inconſiſtent with it. The pure inflammable air, with the water neceſſarily contained in it, would weigh no more than about 30 grains, while the loſs of weight in the charcoal was 94 grains. But to this muſt be added the phlogiſton contained in 392 ounce meaſures of fixed air, which, according to Mr. KIRWAN's proportion, will be nearly 65 grains, and this and the 30 grains will be 95 grains.

The baſis to this fixed air, as well as to the inflammable, muſt have been furniſhed by the *water*; and from this it may be concluded, that the water muſt have been ſo far altered as to be changed into fixed air, which will be thought not to be any great paradox, if it be conſidered that, according to the lateſt diſcoveries, fixed air and water appear to conſiſt of the ſame ingredients, namely, dephlogiſticated and inflammable air. However, in this change of the water we cannot be abſolutely ſure that the ſame proportion of the ingredients is contained, and therefore it cannot be abſolutely determined whether the inflammable air which it contains enters wholly into the fixed air, or not. Farther experiments, or a careful compariſon of theſe with thoſe made by Mr. KIRWAN and others, may perhaps throw ſome light upon this ſubject. Whether the combined fixed air comes wholly from the charcoal, or whether the charcoal only ſupplies the phlogiſton, and the water its baſis, that is, the dephlogiſticated air, deſerves to be inveſtigated.

Before I conclude my account of the experiments with charcoal, I would obſerve, that there is another on which I place ſome dependence, in which, with the loſs of 178 grains of charcoal, and 528 grains of water, I procured 1410 ounce meaſures of air, of which the laſt portion (for I did not examine the reſt) contained one-ſixth part of uncombined fixed air. This was made in an earthen tube glazed on the outſide.

The experiments with *iron* were more ſatisfactory than thoſe with charcoal, being ſubject to leſs variation; and they by no means require us to ſuppoſe that the inflammable air comes from the *water*, but only from the *iron*, as the quantity of water expended, deducting the weight of the air produced, was as nearly as could be expected in experiments of this kind, found in the addition of weight gained by the iron. And though the inflammable air procured in this proceſs is between one-third and one-half more than can be procured from iron by a ſolution in acids, the reaſon may be, that much phlogiſton is retained in the ſolutions, and therefore much more may be expelled from iron, when pure water, without any acid, takes the place of it. I would further obſerve, that the produce of air, and alſo the addition of weight gained by the iron, are much more eaſily aſcertained in theſe experiments than the quantity of water expended in them, on account of the great length of the veſſels uſed in the proceſs, and the different quantities that may perhaps be retained in the worm of the tub; though I did not fail to uſe all the precautions that I could think of, to guard againſt any variation on theſe accounts.

Of the many experiments which I made with *iron*, I ſhall content myſelf with reciting the following reſults. With the addition of 267 grains to a quantity of iron, and the loſs of 336 grains of water, I procured 840 ounce meaſures of inflammable air; and with the addition of 140 grains to another quantity of iron, and the conſumption of 254 grains of water, I got 420 ounce meaſures of air*.

The inflammable air produced in this manner is of the lighteſt kind, and free from that very *offenſive ſmell* which is generally occaſioned by the rapid ſolution of metals in oil of vitriol, and it is extricated in as little time in this way as it is poſſible to do it by any mode of ſolution. On this account it occurred to me, that it muſt be by much the cheapeſt method that has yet been uſed of filling *balloons* with the lighteſt inflammable air. For this purpoſe it will be proper to make uſe of caſt-iron cylinders of a conſiderable length, and about three or four inches, or perhaps more, in diameter. Though the iron tube itſelf will contribute to the production of air, and therefore may become unfit for the purpoſe in time; yet, for any thing that I know to the contrary, the ſame tube may ſerve for a very great number of proceſſes, and perhaps the change made in the inſide ſurface may protect it from any farther action of the water, if the tube be of ſufficient thickneſs; but this can only be determined by experiment.

---

* If the perfect accuracy of the former of theſe experiments may be depended on (and it may always be preſumed, that thoſe in which *little water* is expended are preferable to thoſe in which *more* is conſumed) the water that neceſſarily enters into this kind of inflammable air is about equal in weight to the *phlogiſton* that is in it. But I propoſe to give more particular attention to this ſubject:

Some eſtimate of what may be expected from this method of procuring inflammable air may be formed from the following obſervations. About twelve inches in length of a copper tube, three-fourths of an inch in diameter, filled with *iron turnings* (which are more convenient for this purpoſe than *iron filings*, as they do not lie ſo cloſe, but admit the ſteam to paſs through their interſtices)

ſtices) when it was heated, and a ſufficient quantity of ſteam paſſed through it, yielded thirty ounce meaſures of air in fifty ſeconds; and eighteen inches of another copper tube, an inch and a quarter in diameter, filled and treated in the ſame manner, gave two hundred ounce meaſures in one minute and twenty-five ſeconds; ſo that this larger tube gave air in proportion to its ſolid contents compared with the ſmaller; but to what extent this might be depended upon I cannot tell. However, as the heat penetrates ſo readily to ſome diſtance, the rate of giving air will always be in a greater proportion than that of the ſimple diameter of the tube.

The following experiment was made with a view to aſcertain the quantity of inflammable air that may be procured in this way from any given quantity of iron. Two ounces of iron, or 960 grains, when diſſolved in acids, will yield about 800 ounce meaſures of air; but treated in this manner it yielded 1054 ounce meaſures, and then the iron had gained 329 grains in weight, which is little ſhort of one-third of the weight of the iron.

Conſidering how little this inflammable air weighs, *viz.* the whole 1054 ounce meaſures not more than 63 grains, and the difficulty of aſcertaining the loſs of water to ſo ſmall a quantity as this, it is not poſſible to determine, from a proceſs of this kind, how much water enters into the compoſition of the inflammable air of metals. It would be more eaſy to determine this circumſtance with reſpect to the inflammable air of charcoal, eſpecially by means of the experiment made with a burning lens *in vacuo.* In this method two grains of charcoal gave at a medium thirteen ounce meaſures of inflammable air, which, in the proportion of 30 ounce meaſures to 8 grains, will weigh 3.3 grains; ſo that water in the compoſition of this kind of inflammable air is in the proportion of 1.3 to 2, though there will be ſome difficulty with reſpect to the fixed air intimately combined with this kind of inflammable air.

Since iron gains the ſame addition of weight by melting in *dephlogiſticated air*, and alſo by the addition of *water* when red-hot, and becomes, as I have already obſerved, in all reſpects the ſame ſubſtance, it is evident, that this *air* or *water*, as exiſting in the iron, is the very ſame thing; and this can hardly be explained but upon the ſuppoſition that water conſiſts of two kinds of air, *viz.* inflammable and dephlogiſticated. I ſhall endeavour to explain theſe proceſſes in the following manner.

When iron is melted in dephlogiſticated air, we may ſuppoſe that, though part of its phlogiſton eſcapes, to enter into the compoſition of the ſmall quantity of fixed air which is then procured, yet enough remains to form *water* with the addition of the dephlogiſticated air which it has imbibed, ſo that this *calx* of iron conſiſts of the intimate union of the pure *earth of iron* and of *water*; and therefore when the ſame calx, thus ſaturated with water, is expoſed to heat in inflammable air, this air enters into it, deſtroys the attraction between the water and the earth, and revives the iron, while the water is expelled in its proper form.

Conſequently, in the proceſs with *ſteam*, nothing is neceſſary to be ſuppoſed but the entrance of the water, and the expulſion of the phlogiſton belonging to the iron, no more phlogiſton remaining in it than what the water brought along with it, and which is retained as a conſtituent part of the water, or of the new compound.

Having procured water from the ſcales of iron (which I muſt again obſerve is, in all reſpects, the ſame ſubſtance with iron melted in dephlogiſticated air, or ſaturated with ſteam by means of heat) and having thereby converted it into perfect iron again, I did not entertain a doubt but that I ſhould be able to produce the ſame effect by heating it with charcoal in a retort; and I had likewiſe no doubt but I ſhould be able to extract the additional weight which the iron had gained (*viz.* one-third of the whole) in *water*. In the former of theſe conjectures I was right; but with reſpect to the latter, I was totally miſtaken.

Having made the ſcales of iron, and alſo the powder of charcoal very hot, previous to the experiment, ſo that I was ſatisfied that no air could be extracted from either of them ſeparately by any degree of heat, and having mixed them together while they were hot, I put them into an earthen retort, glazed within and without, which was quite impervious to air. This I placed in a furnace, in which I could give it a very ſtrong heat; and connected with it proper veſſels to condenſe and collect the water which I expected to receive in the courſe of the proceſs. But, to my great ſurpriſe, not one particle of *moiſture* came over, but a prodigious quantity of *air*, and the rapidity of its production aſtoniſhed me; ſo that I had no doubt but that the weight of the air would have been equal to the loſs of weight both in the ſcales and in the charcoal; and when I examined the air, which I repeatedly did, I found it to contain one-tenth of fixed air, and the inflammable air, which remained when the fixed air was ſeparated from it, was of a very remarkable kind, being quite as heavy as common air. The reaſon of this was ſufficiently apparent when it was decompoſed by means of dephlogiſticated air; for the greateſt part of it was fixed air.

The theory of this proceſs I imagined to be, that the phlogiſton from the charcoal reviving the iron, the water with which it had been ſaturated, being now ſet looſe, affected the hot charcoal as it would have done if it had been applied to it in the form of *ſteam* as in the preceding experiments; and therefore the air produced in theſe two different modes have a near reſemblance to each other, each containing fixed air, both combined and uncombined, though in different proportions; and in both the caſes I found theſe proportions ſubject to variations. in one proceſs with charcoal and ſcales of iron, the firſt produce contained one-fifth of uncombined fixed air, the middle part one-tenth, and the laſt none at all. But in all theſe caſes the proportion of combined fixed air varied very little.

Why *air* and not *water* ſhould be produced in this caſe, as well as in the preceding,

ing, when the iron is equally revived in both, I do not pretend perfectly to underſtand. There is, indeed, an obvious difference in the circumſtances of the two experiments; as in that with charcoal the phlogiſton is found in a combined ſtate; whereas in that of inflammable air, it is looſe, or only united to water; and perhaps future experiments may diſcover the operation of this circumſtance*.

---

* This experiment ſeems to be deciſive againſt the hypotheſis of Mr. LAVOISIER, and others, who ſay that the inflammable air procured by means of iron and charcoal comes from the water, and who think that by this means they can exclude phlogiſton. For, according to them, neither the ſcales of iron, nor the charcoal, contain phlogiſton, or any thing from which inflammable air can be made, but are merely ſubſtances capable of imbibing pure air, and thereby ſetting at liberty the inflammable air contained in the water. As to the ſcales of iron, they were only iron ſaturated with dephlogiſticated air. But had this been the caſe, there was nothing in either of the materials made uſe of in this experiment from which the inflammable air could poſſibly come, there being no *water* contained in either of them. But ſuppoſing the reality of phlogiſton, and its conſtituting a part of metals, of charcoal, and of inflammable air, the experiment is very intelligible.

There is ſome analogy between the experiment of the calx of iron imbibing inflammable air, and the iron itſelf imbibing dephlogiſticated air. In the former caſe *water* is produced, and in the latter *fixed air*. However, this caſe of iron imbibing dephlogiſticated air, more nearly reſembles the caſe of the *blood in the lungs* imbibing the ſame kind of air, and in both the caſes as dephlogiſticated air is imbibed, fixed air is formed. This, therefore, ſeems to be a confirmation of the concluſion which I drew from my former experiments on blood, *viz.* that it parts with phlogiſton in reſpiration. Only I would now add, that at the ſame time that it parts with phlogiſton it takes in dephlogiſticated air, which makes the caſe perfectly ſimilar to that of the experiment with *iron*, which likewiſe parts with phlogiſton to form fixed air, at the ſame time that it imbibes dephlogiſticated air in contact with which it is fuſed.

I propoſe to reſerve for a future communication the continuation of theſe experiments, containing an account of the application of the ſame proceſs to other ſubſtances; but it may not be amiſs juſt to mention a few of the *general reſults*, and thoſe which have the neareſt connexion with the experiments recited above.

After having tranſmitted ſteam in contact with *charcoal* and *iron* in a copper tube, I propoſed to do the ſame with other ſubſtances containing phlogiſton, and I began with *bones*, which were burnt black, and had been ſubjected to an intenſe heat, covered with ſand, in an earthen retort. From three ounces of bones thus prepared, and treated as I had done the charcoal, I got 840 ounce meaſures of air, with the loſs of 288 grains of water. The bones were by this means made perfectly white, and had loſt 110 grains of their weight. As the air ceaſed to come a conſiderable time before all the water had been tranſmitted through the tube containing them, I concluded that the air was formed from the phlogiſton contained in the bones, and ſo much water as was neceſſary to give it the form of air.

This air differs conſiderably from any other kind of inflammable air, being in ſeveral reſpects a medium between that from charcoal and that from iron. It contains about one-fourth of its bulk of uncombined fixed air, but not quite one-tenth intimately combined with the remainder. The water that came over was blue, and pretty ſtrongly alkaline, which muſt have been occaſioned by the volatile alkali not having been intirely expelled from the bones in the former proceſs, and its having in part diſſolved the copper of the tube in which the experiment was made.

I ſubjected to the ſame proceſs a variety of ſubſtances that are ſaid not to contain phlogiſton, but I was never able to procure inflammable air by means of them; which ſtrengthens the hypotheſis of the principal element in the conſtitution of this air having been derived from the ſubſtance ſuppoſed to contain phlogiſton, and therefore that phlogiſton is a real ſubſtance capable of aſſuming the form of air by means of water and heat.

The experiments above-mentioned relating to iron were made with that kind which is *malleable*; but I had the ſame reſult when I made uſe of ſmall nails of *caſt iron*, except that theſe were firmly faſtened together after the experiment, the ſurfaces of them being cryſtallized, and the cryſtals mixing with each other, ſo that it was with great difficulty that they could be got out of the tube after the experiment; and in general the ſolid parts of the nails were broken before they were ſeparated from each other. Indeed the pieces of malleable iron adhered together after the experiment, but by no means ſo firmly.

Caſt iron annealed (by being kept red-hot in charcoal) is remarkably different from the caſt iron which has not undergone that operation, eſpecially in its being, to an extraordinary degree, more ſoluble in acids. With the turnings of annealed caſt iron I made the following experiment. From 960 grains of this iron, and with the loſs of 480 grains of water, I got 870 ounce meaſures of inflammable air, and tranſmitting ſteam through them a ſecond time, I got 150 ounce meaſures more. The iron had then gained 246 grains in weight, and the pieces adhered firmly together; but being thin they were eaſily broken and got out of the tube, whereas it had required a long time, and a ſharp ſteel inſtrument, to clear the tube of the caſt-iron nails.

Willing to try the effect of heating iron, and other ſubſtances, in all the different kinds of air, without any particular expectation, I found that iron melted more readily in *vitriolic acid* air than in dephlogiſticated air, the air was diminiſhed as rapidly, and the inſide of the veſſel was covered with a *black ſooty matter*, which when expoſed to heat, readily ſublimed in the form of a white vapour, and left the glaſs quite clean. The iron, after the experiment, was quite brittle, and muſt, I preſume, be the ſame thing with iron that is *ſulphurated*; but I did not particularly examine it. Of ſeven ounce meaſures of vitriolic acid air, in one of theſe experiments, not more than three-tenths of an ounce meaſure remained; of this two-thirds was fixed air, and the reſiduum of this was inflammable. I had put three of ſuch reſiduums together, in order to make the experiment with the greater certainty.

Having tranſmitted *ſteam*, or the vapour of water, through a copper tube, I was willing to try the effects of *ſpirit of wine* through the ſame tube when red-hot, having before procured inflammable air by ſending the ſame vapour through a red-hot tobacco-pipe. In this caſe, the vapour of the ſpirit of wine had no ſooner entered the hot copper tube, than I was perfectly aſtoniſhed at the rapid production of air. It reſembled the blowing of a pair of bellows. But I had not uſed four ounces of the ſpirit of wine before I very unexpectedly found, that the tube was perforated in ſeveral places; and preſently afterwards it was ſo far deſtroyed, that in attempting to remove it from the fire it actually fell in pieces. The inſide was full of a black ſooty matter reſembling lamp-black.

Upon this I had recourſe to *earthen tubes*, and found, that by melting copper and other metals in them, and tranſmitting the vapour of ſpirit of wine in contact with them, different ſubſtances were formed according to the metals employed. The new ſubſtances hereby formed may be ſaid to be the ſeveral metals ſuper-ſaturated with phlogiſton, and may perhaps not be improperly called the *charcoal of the metals*.

That this appellation is not very improper, may appear from theſe ſubſtances yielding inflammable air very copiouſly when they are made red-hot, and the ſteam of *water* is tranſmitted in contact with them, juſt as when the charcoal of wood is treated in the ſame manner. The detail of theſe experiments I reſerve for another communication, as alſo thoſe of the converſion of *ſpirit of wine*, *æther*, and *oil*, into different kinds of inflammable air, by tranſmitting them, in vapour, through hot earthen tubes. In the mean time, I ſhall think myſelf happy if the communication of the preceding experiments ſhall give any ſatisfaction to the Members of the Society[*].

---

[*] An account of theſe experiments will be found in the preſent volume. They were not completed ſoon enough to ſend to the Society before the publication of this volume.

POSTSCRIPT.

BEFORE I cloſe this paper, I wiſh to make a few *general inferences* from the principal of the experiments above-mentioned, eſpecially relating to the proportional quantity of phlogiſton contained in *iron* and *water*.

When any quantity of iron is melted in dephlogiſticated air, it imbibes the greateſt part of it, and gains an addition of weight very nearly equal to that of the air imbibed. Thus the abſorption of twelve ounce meaſures of dephlogiſticated air gave an addition of ſix grains to the piece of iron which had been melted in it. But there was always a quantity of *fixed air* produced in this proceſs; and on the ſuppoſition that this air conſiſts of the union of dephlogiſticated and inflammable air, it proves that the dephlogiſticated air which enters the iron expels more phlogiſton than is neceſſary to conſtitute an equal weight of water, ſo that *water* does not contain ſo much phlogiſton as *iron*; but the difference is not very conſiderable.

Admitting Mr. KIRWAN'S concluſion, *viz.* that 100 cubic inches of fixed air contain 8,357 grains of phlogiſton, the .13 of an ounce meaſure of fixed air, which (in an experiment recited in theſe papers) was found in the reſiduum of ſeven ounce meaſures of dephlogiſticated air abſorbed by iron, would not have contained more than .01 of a grain of phlogiſton, or about .16 of an ounce meaſure of inflammable air. Then, as the abſorption of 12 ounce meaſures of dephlogiſticated air occaſioned an addition of 6 grains to the weight of the iron which had abſorbed it, the abſorption of ſeven ounce meaſures muſt have occaſioned the addition of 3.5 grains to the iron which had imbibed it. But the ſame addition of weight to iron given by *ſteam* (which carries its own inflammable air along with it) would have expelled near 12 ounce meaſures of inflammable air; conſequently, about 10 ounce meaſures

ſures of inflammable air (or the phlogiſton requiſite to form it) muſt, in the former experiment, have been retained in the iron, in order to compoſe the *water* which was now made by the union of the dephlogiſticated air imbibed by the iron and the phlogiſton contained in it: and therefore the proportion between the quantity of phlogiſton in *iron* to that which is contained in an equal weight of water, may be about 12 to 10, or more accurately to 10.4.

Had no fixed air at all been found in the reſiduum above-mentioned, it might have been concluded, that water had contained the very ſame proportion of phlogiſton with iron. Since when iron that has been ſaturated with dephlogiſticated air is heated in inflammable air (in which proceſs an equal weight of water is produced, and the loſs of weight in the iron is equal to that of ſuch a quantity of dephlogiſticated air as would have been one-half of the bulk of the inflammable air which diſappears in that proceſs) it might have been concluded, that one-fifth of any quantity of water had been inflammable air.

For, neglecting the difference between the weight of dephlogiſticated and common air, which is not conſiderable, and eſtimating the latter at $\frac{1}{800}$th part that of water, and inflammable air at one-tenth of the weight of common air, an ounce meaſure of dephlogiſticated air will weigh .6 of a grain, and two ounce meaſures of inflammable air will weigh .12 of a grain, which numbers are to each other as 5 to 1 *.

---

* It appears from the proſecution of theſe experiments, that the water which is found on heating the ſcales of iron in inflammable air, is not formed by the dephlogiſticated air expelled from them uniting with the inflammable air in the veſſel, but was the water previouſly contained in the ſcales, which is made to quit its place by the introduction of the phlogiſton from the inflammable air; yet that water carries out with it not much leſs phlogiſton than was taken in by the iron, and a little more muſt be allowed for that water which was neceſſary to make inflammable air, and which could not enter the iron when it was revived; ſo that, on the whole, the phlogiſton in the water that is found after the proceſs muſt be very nearly the ſame quantity that is imbibed by the iron, and the

Though, in conſequence of the ſmall quantity of fixed air which is found in the proceſs of melting iron in dephlogiſticated air, this concluſion is not accurate, it is pretty nearly ſo; and it is remarkable that, upon this ſuppoſition, about as much inflammable air is expelled from iron when water is combined with it, as the water itſelf brings along with it, as an eſſential ingredient in its compoſition. For in one experiment 296 grains added to the weight of a quantity of iron by ſteam, made it to yield about 1000 ounce meaſures of inflammable air. This would weigh 60 grains, and one-fifth of the 296 grains of water will be 59.2 grains. Again, 267 grains added to iron by ſteam made it to yield 840 ounce meaſures of inflammable air, which would weigh 50.4 grains, and one-fifth of the 267 would be 53.4 grains.

---

the water is nearly the ſame that would have been produced, on the ſuppoſition of its being made from dephlogiſticated air expelled from the ſcales uniting with the inflammable air in the veſſel.

When the experiments on the melting of iron in dephlogiſticated air ſhall be repeated on a larger ſcale, which it will not be difficult to do by the help of a larger burning lens than I am at preſent poſſeſſed of, it will be eaſy to reduce theſe calculations to a a greater certainty. All that I can do at preſent is to approximate to ſuch general concluſions as I have mentioned; but they are of ſo much conſequence in philoſophy, that it will certainly be well worth while to aſcertain them with as much accuracy as poſſible. Nice calculations would be ill beſtowed on the imperfect *data* which I am as yet able to furniſh. Attention muſt alſo be given to the quantity of water contained in inflammable air from iron; which not being yet aſcertained is not conſidered in theſe inferences. I wiſh only to hint in this Poſtſcript, that ſome important concluſions ſeem to be nearly within our reach.

SECTION IV.

Farther Obſervations relating to the Compoſition of Water.

THAT dephlogiſticated and inflammable air, at leaſt with the element of *heat*, conſtitute *water*, ſeems to be clearly inferred from the experiments in the preceding ſection. Some difficulties, however, have occurred reſpecting this theory, from my ſubſequent experiments, which I ſhall propoſe with the ſame fidelity as I have done thoſe which favour the hypotheſis. But as I mean to throw into this ſection all the obſervations that I have made upon the ſubject, I ſhall firſt recite ſome experiments which perfectly agree with the former, and evidently lead to the ſame concluſion. Some of them are thoſe of which only the general reſult was given before.

I have obſerved, that, when that iron which has been melted in the open air (or that which has been altered by the paſſing of ſteam over it red-hot) was heated by a burning lens in inflammable air, the air diſappeared, and a conſiderable quantity of water was produced. I had the ſame reſult with *ſcales of copper*. Theſe ſcales became of a genuine copper colour in this proceſs; ſo that I had no doubt of their being copper revived. The water was ſo copious, that when only 3½ ounce meaſures of air were abſorbed, the water formed in drops on the inſide of the veſſel, and ſome of them ran down it.

I alſo procured water when I decompoſed dephlogiſticated and inflammable air from iron by the electric ſpark in a cloſe veſſel, which is an experiment ſimilar to thoſe that were made by Mr. LAVOISIER, at Paris. I put 3.75 ounce meaſures of a mixture of air, of which one-third was dephlogiſticated, and two-thirds inflammable from iron, in the cloſe veſſel; and after the exploſion I found in it one grain of moiſture. The dephlogiſticated air in this mixture would have weighed a little more than 0.75 of a grain. But repeating this experiment with half as much dephlogiſticated as inflammable air, I could not perceive any water after the experiment. Neither was there any fixed air formed in it. Again, uſing inflammable air from wood, in the ſame proportion from 3.8 ounce meaſures of the mixture, I got 0.8 of a meaſure of fixed air, which was actually abſorbed by lime-water. Some *water* alſo was evidently produced.

Uſing more precautions to exclude all water from either of the two kinds of air before the experiment (both the dephlogiſticated air, which was from nitre, and the inflammable air, which was from charcoal, being from the firſt received in mercury, and always confined by it) I ſtill found a little water after the exploſion.

I varied this experiment by producing the inflammable air in the dephlogiſticated air as follows. Into a veſſel containing dephlogiſticated air confined by mercury, I introduced a piece of perfect charcoal, as hot from the fire as I could bear to handle it, and threw upon it the focus of the lens, ſo that a quantity of the air was imbibed; but I could not perceive that any moiſture was formed. Afterwards, when I reſumed the proceſs, the air which the charcoal had imbibed was expelled again, and very little more was abſorbed. From 7 ounce meaſures there remained 4, of which more than 2 was pure fixed air. No water could be expected after this proceſs. For even had there been a ſmall quantity of moiſture in the veſſel, it would have been abſorbed by the charcoal, and have enabled it to yield inflammable air. The phlogiſton the charcoal contained uniting with the dephlogiſticated air, free from moiſture, formed, I preſume, the fixed air that was found after this proceſs.

But the greateſt difficulty that occurred with reſpect to the preceding theory of the conſtitution of water, aroſe from my never having been able to procure any water when I revived mercury from red precipitate in inflammable air, or at leaſt not more than may be ſuppoſed to have been contained in the inflammable air, as an extraneous ſubſtance.

In order to make the experiments with the *ſcales of iron*, and that with the *red precipitate*, as much alike as poſſible, and that I might compare them to the greateſt advantage, I made them immediately one after the other, with every circumſtance as nearly as I could the ſame. The inflammable air was the ſame in both the experiments, and both the ſcales of iron, and the red precipitate, were made as dry as poſſible. They were heated in veſſels of the ſame ſize and form, and equally confined by dry mercury. And yet when I heated the former, water was formed as copiouſly as I have deſcribed it before, viz. actually running down the inſide of the veſſel in drops, though only 4 ounce meaſures of inflammable air were abſorbed. But though I heated the red precipitate till 8 ounce meaſures of the inflammable air was abſorbed, and only three-fourths of an ounce meaſure of air remained, there was hardly any ſenſible quantity of water produced, certainly not one-tenth of what appeared in the experiment with the ſcales of iron.

There was this difference, however, in the two reſults. In what remained from the experiment with red precipitate, I at this time perceived a ſlight appearance of *fixed air*, whereas there was none in what remained from the ſcales of iron. The reſiduum alſo from the red precipitate had in it a very ſmall portion of dephlogiſticated air. For being mixed with an equal meaſure of nitrous air, the ſtandard of it was 1.8. I muſt alſo obſerve that the inflammable air diſappeared much more rapidly in the proceſs with red precipitate, than in that with the ſcales of iron.

Fixed air, however, was no neceſſary reſult in this experiment*. For I particularly obſerved, that when, upon another occaſion, I reduced 8 ounce meaſures of inflammable air to 0.22 of a meaſure, I found no more than what I judged to be much leſs than a grain of water, and without any appearance of fixed air whatever. Again, I reduced 6 ounce meaſures of inflammable air in this proceſs to 0.42 of a meaſure, without producing any more water than before, or any fixed air at all. I alſo had the ſame reſult in uſing the black powder of lead and mercury, inſtead of red precipitate; reducing 6.5 ounce meaſures to 0.4 of a meaſure, without finding any very ſenſible quantity of water, or any fixed air.

---

* It will be ſeen in the ſection relating to inflammable air, that there is a ſlight appearance of fixed air, in the decompoſition of that inflammable air which is produced by means of *ſpirit of ſalt*; and, not being aware of this at the time of making theſe experiments, I took no account how the air was made.

In this experiment there can be no doubt but that the dephlogiſticated air diſlodged from the red precipitate, united with the inflammable air in the veſſel; and as no *water* equal to the weight of the two kinds of air was produced, they muſt have formed ſome more *ſolid ſubſtance*, which, in the ſmall quantities I was obliged to uſe, could not be found.

It may be clearly inferred from this experiment, that red precipitate is a ſubſtance by no means ſimilar to the ſcales of iron, as the latter appears to contain within itſelf all the elements of water; and therefore as the inflammable air enters into it, a quantity of water, equal in weight to the loſs ſuſtained by the ſcales in their revival in the form of iron, is found in the recipient. Whereas in the experiment with the red precipitate, there is certainly no more water found, than may be ſuppoſed to have been contained in the inflammable air which diſappeared, or to have been lodged without being perceived among the particles of the precipitate itſelf. Conſequently the ſcales of iron muſt be conſidered as the calx of iron united to *water*, and red precipitate as mercury, united to *dephlogiſticated air*, or rather, perhaps, as Mr. Kirwan ſuppoſes, to *fixed air*, the phlogiſton belonging to which revives the mercury, while its other component part, the dephlogiſticated air, is ſet at liberty, forming an union with the element of *heat*.

The difficulty with reſpect to what becomes of the two kinds of air, was not leſſened by the attempts which I made to collect all that I could from repeated decompoſitions of inflammable and dephlogiſticated air in a cloſe veſſel.

As I had produced water in this proceſs when I made no more than a ſingle exploſion at a time, I thought that by continuing to make exploſions in the ſame veſſel, the water would not fail to accumulate, till I might collect what quantity I pleaſed; and I intended to have collected a conſiderable part of an ounce. And as I ſhould know exactly what quantity of air I decompoſed, I had no doubt of being able to aſcertain the proportion that the *water* and *air* bore to each other.

With this view I made a mixture of a large quantity of air, one-third dephlogiſticated, and two-thirds inflammable from iron and oil of vitriol. But though I had a ſenſible quantity of water at the firſt exploſion (in each of which was uſed between four and five ounce meaſures of the mixture of air)

air) I was ſurpriſed to perceive no very ſenſible increaſe of the quantity of water on repeating the exploſions. Having, therefore, expended 48 ounce meaſures of the mixture, I diſcontinued the proceſs; and collecting the water with all the care that I could, I found no more than three grains, when there ought to have been eleven.

In this proceſs the inſide of the veſſel was always very black after each exploſion, and when I poured in the mercury after the exploſion, though there was nothing viſible in the air within the veſſel, there iſſued from the mouth of it a *denſe vapour*. This was even the caſe, though I waited ſo much as two minutes after any exploſion before I proceeded to put in more mercury in order to make another; which if the vapour had been *ſteam*, would have been time more than ſufficient to permit it to condenſe into water. I even perceived this vapour when I had a quantity of water in the veſſel, and the exploſion was conſequently made over it, as well as in contact with the ſides of the veſſel which were wetted with it; ſo that as this vapour had paſſed through the whole body of water when the veſſel was inverted, it is probable that it muſt have conſiſted of ſomething elſe than mere *water*. But I was never able to collect any quantity of it, though it muſt have been ſomething produced by the union of the two kinds of air.

In order to collect a quantity of the matter that formed this vapour, I contrived the following apparatus. In a cork (*a*) Fig. 3, with which I could ſhut the orifice of the ſtrong glaſs veſſel (*b*) in which the exploſions were made, I had two perforations. Through one of theſe (*c*) I poured the mercury, by means of a glaſs funnel; but into the other was introduced a glaſs tube, which, being bended, was inſerted, by means of a cork, into a thin glaſs veſſel (*d*) and went almoſt to the bottom of it. A ſmall hole was alſo made in the cork, to permit the air to go out. Conſequently, all the air that remained in the ſtrong glaſs veſſel, with whatever vapour it might contain, muſt, as I poured in the mercury, neceſſarily paſs through the glaſs tube, and be diffuſed through the thin glaſs veſſel; in which I imagined that all its contents, fluid or ſolid, muſt be depoſited. However, though I repeated the experiment ſeveral times with this apparatus, making about twenty exploſions in each, I could not find any depoſit in the veſſel, beſides a ſmall quantity of *water*; which, added to the water collected in the ſtrong veſſel, came far ſhort of the weight of the air that was decompoſed.

All the conjecture that I can advance, in order to explain this phænomenon, is that, ſince *ſoot* yields pure air, as will appear in the courſe of this volume, part of the ſoot is formed by the union of the dephlogiſticated air in the atmoſphere and the inflammable air of the fuel. But *ſmoke*, which contains much *ſoot*, is ſoon diſperſed, and becomes inviſible in the open air. Such, therefore, may be the caſe here. The ſoot formed by the union of the two kinds of air may be diffuſed through the air, in the veſſel in which they are exploded, and be carried inviſibly into the common atmoſphere, which may account for my not being able to collect any quantity of it in this apparatus.

Hoping to ſucceed better in collecting this volatile matter by means of a quantity of water incumbent upon the mercury, in the ſtrong glaſs veſſel in which the exploſions were made (though I had found that part of it could eſcape through the water) I decompoſed a great quantity of the two kinds of air in theſe circumſtances; and preſently found that the water became very cloudy, and was at length full of a blackiſh matter. This I collected, and found that it remained perfectly black upon the earthen veſſel in which the water containing it was evaporated; which would not have been the caſe if the blackiſh matter in the water had been that *powder of mercury*, which is produced by agitating it in pure water. For that black maſs always became white running mercury the moment the water was evaporated from it. Could I have collected a ſufficient quantity of this black matter, I might have ſatisfied myſelf whether it was a proper *ſoot* or not.

Mr. WARLTIRE firſt obſerved this cloudineſs in a veſſel in which he burned inflammable air; but it is remarkable that ſometimes I got it repeatedly in theſe exploſions, all the inſide of the veſſel becoming quite black after the exploſion, and at other times I have not been able to get this appearance at all; ſo that I am not yet able to determine on what it depends. At one time, having the inſide of the ſtrong glaſs tube made very black with theſe exploſions, I let it remain a day or two expoſed to the common air, when the blackneſs diſappeared, leaving the inſide of the veſſel covered with ſmall globules of white mercury. It ſeems, therefore, that part of the phlogiſton of the inflammable air ſometimes attaches itſelf to the vapour of mercury, diffuſed inviſibly through the ſpace within the veſſel, and that it quits it to unite to the air of the atmoſphere.

That water in great quantities is ſometimes produced from burning inflammable and dephlogiſticated air is evident from the experiments of Mr. CAVENDISH and Mr.

LAVOISIER. I have alſo frequently collected conſiderable quantities of water in this way, though never quite ſo much as the weight of the two kinds of air decompoſed. My apparatus for this purpoſe was the following. Into the mouth of a large glaſs balloon (*a*) Fig. 4, I introduced a tube from the orifice of which there continually iſſued inflammable air, from a veſſel containing iron and oil of vitriol. This being lighted, continued to burn like a candle. Preſently after the lighting of it, the inſide of the balloon always became cloudy, and the moiſture ſoon gathered in drops, and ſettled in the lower part of the balloon. To catch what might iſſue in the form of vapour, in the current of air through the balloon, I placed the glaſs tube (*b*) in which I always found ſome water condenſed. It is very poſſible, however, that in both theſe modes of experimenting, the water may be converted into a kind of vapour, which is very different from *ſteam*, and capable of being conveyed a great way through air, or even water, without condenſation, along with the air with which it is mixed; and on this account it may not be poſſible, in either of theſe modes of experimenting, to collect *all* the water into which the two kinds of air may be converted. The nature of this kind of vapour into which water may be changed, and which is not readily condenſed by cold, is very little underſtood, but well deſerves the particular attention of philoſophers*. Even mercury will evaporate, ſo as to loſe weight, in a degree of heat below that of boiling water.

That the water collected in the balloon comes from the decompoſition of the air, and not from the freſh air circulating through it, was evident from placing balls of hot iron in the place of the flame, and finding that, though the balloon was as much heated by them as by the flame of the burning of the inflammable air, and conſequently there muſt have been the ſame current of the external air through it, no moiſture was found in the balloon.

When, in this manner, I burned inflammable air from pure iron, the water I collected was perfectly free from acid, and the inſide of the balloon was quite clear, but when I uſed *ſulphurated iron*, there was a denſe white cloud that filled the inſide of the balloon. There was alſo a ſtrong ſmell of vitriolic acid air, and the water collected was ſenſibly acid to the taſte.

---

* Mr. SAUSSURE has made ſome valuable obſervations on this ſubject.

Having found that water is an eſſential ingredient in the conſtitution of inflammable air, at leaſt as produced from iron, it ſtill remained to be determined whether, when a calx is revived, and the metal formed, the pure phlogiſton only entered the calx, or, together with it, that *water* which was neceſſary to its form of inflammable air.

In order to aſcertain this, I frequently revived dry calces of lead in dry inflammable air, and examined the appearances of moiſture afterwards. But notwithſtanding all the attention that I gave to the proceſs, I could not be abſolutely certain, whether more moiſture was left in the veſſel, than might have exiſted *extraneouſly* in the inflammable air, or whether, when the phlogiſton was abſorbed, it left behind it any water that had been eſſential to it, as inflammable air. Appearances were ſuch as ſometimes inclined me to think that every thing which conſtitutes inflammable air goes into a calx, in order to form the metal; ſo that if this, though a compound thing, be called *phlogiſton*, it will ſtill be true that phlogiſton and inflammable air are the ſame thing; but, on the whole, I rather think that the water which was eſſential to the conſtitution of inflammable air was left behind.

That water, however, may exiſt in bodies in a *combined ſtate*, without appearing to be water, we know in many caſes; but it is in nothing more evident than in the *ſcales of iron*, than which no ſubſtance can have leſs the appearance of containing water.

But not to give a mere *opinion*, I ſhall recite the particulars of a few *experiments*, which I made with the view above-mentioned. In 6 ½ ounce meaſures of inflammable air from iron, I revived lead till it was reduced to 1 ½ ounce meaſure, care having been taken to make every thing as dry as poſſible. Some moiſture, however, did appear, perhaps more than half a grain; but as this air had been confined by water, it was no more than might have been contained in it as an extraneous ſubſtance. It ought alſo to be conſidered, that it muſt be exceedingly difficult to expel all moiſture by mere heat from ſuch a powdery ſubſtance as the yellow calx of lead, without reviving the metal. All chemiſts well know how firmly moiſture adheres to many ſubſtances, with which it does not properly unite, and how much heat is neceſſary to ſeparate them.

Again, in 6½ ounce meaſures of inflammable air from iron, I revived lead till there remained 0.9 of a meaſure, and there was hardly any more moiſture than I had reaſon to think might have been in the veſſel, independently of what was contained in the inflammable air; and in order to enable myſelf to judge of this, I melted an equal quantity of the ſame minium, under a dry glaſs veſſel with common air, when a little moiſture appeared on the inſide of the glaſs, about as much, I thought (for I could only judge by my eye) as when I had revived the lead from that minium in inflammable air. The quantity of lead revived was only 16 grains, but a good deal of the minium had been made black in the proceſs.

Laſtly, I expoſed ſome calx of lead to the heat of the lens in inflammable air, received immediately from the veſſel in which it was generated from iron and oil of vitriol, becauſe this contains leſs water than that which has been received in water and confined by it; and when 6 or 7 ounce meaſures of the air were abſorbed, I could not ſuppoſe, from the appearance, that the water could be more than a quarter of a grain. However, when I repeated the experiment once more, I thought there might be about half a grain of water, which is more than I can well account for, without ſuppoſing that the water which was neceſſary to the conſtitution of inflammable air, and which I ſuppoſe to be about half its weight, was left behind when the pure phlogiſton revived the calx. This, therefore, is the opinion to which I am inclined; ſo that I do not think that any water enters into the conſtitution of any of the metals.

SECTION V.

Of the Production of Inflammable Air from different Subſtances.

IT is probable, that every ſubſtance which contains *phlogiſton* may be made to yield inflammable air. But for this purpoſe they require different modes of treatment, according to their reſpective natures. If the ſubſtances be *fluid,* heat applied to them directly makes no change in their conſtitution; but when they are made to paſs, in the form of *vapour*, through tubes previouſly made red-hot, in which they are neceſſarily expoſed to a red heat themſelves, they are readily decompoſed; and the quantity of inflammable air that was yielded by ſome of them, in this mode of treatment, appeared to me rather extraordinary.

I began theſe experiments with *ſpirit of wine*, having an apparatus proper to receive any *water*, or other fluid, that might be formed, or condenſed, in the proceſs. From two ounce meaſures of ſpirit of wine, which was made to paſs, in vapour, through a red-hot earthen tube, I got about 1900 ounce meaſures of air, which was all inflammable, without any mixture of fixed air in it, and which burned with a lambent blue flame. Thirty ounce meaſures of this air weighed eight grains leſs than an equal quantity of common air. In this proceſs I collected 0.35 of an ounce meaſure of water.

In this experiment the air would have
weighed - - - 633 grains
the watery reſiduum 168
801

and the ſpirit of wine would have weighed 821; ſo that the produce was pretty nearly

what

what might have been expected from the materials, the nature of the proceſs conſidered.

I then proceeded to ſubject to the ſame proceſs a quantity of *vitriolic æther*; and making an ounce meaſure of it paſs through the hot earthen tube, almoſt filled with pieces of broken retorts, or crucibles (in order to make a greater quantity of red-hot ſurface) I collected one tenth of an ounce meaſure of water, and 740 ounce meaſures of air, all inflammable, without any mixture of fixed air. It burned with a large lambent white flame, like that of wood in a common fire, and would not explode with any mixture of dephlogiſticated air. Twenty-nine ounce meaſures of this air weighed five grains leſs than an equal bulk of common air.

In the next place, I made ſome vapour of *ſpirit of turpentine* paſs through the hot earthen tube, and procured from it a quantity of inflammable air, that was very turbid, like black ſmoke. But the black matter contained in it was ſoon depoſited on the ſurface of the water in which it was received. This alſo contained no fixed air, and burned with a lambent flame, but much leſs luminous than that in the preceding experiment. The ſmell of this air was ſo exceedingly offenſive, that, the apparatus being a little deranged, I diſcontinued the proceſs before I had aſcertained the quantity of air, and without collecting any water, which I ſuppoſe would have been given. Thirty ounce meaſures of this air weighed eight grains leſs than an equal quantity of common air.

I did not repeat this experiment with *olive oil*, being apprehenſive that the proceſs would be even more offenſive than that with the ſpirit of turpentine, and nothing material depending upon it. But, upon another occaſion, I mixed an ounce of olive oil with 874 grains of *calcined whiting*; and ſubjecting it to a red heat in an earthen retort, I got from it near 300 ounce meaſures of air, and ſhould probably have got much more, if there had been more whiting in proportion to the oil. The firſt portion of this air burned with a large white flame, and the laſt with a ſlight lambent blue one, exactly reſembling the varieties in the proceſs for extracting air from wood; ſo that there can be no doubt, but that it is the *oil* in the wood that gives the air. That excellent philoſopher, Mr. Volta, was the firſt who hit upon this method, or a ſimilar one, of getting inflammable air from *oil*; and he has given a large account of its peculiar properties.

From other experiments that I made, it appears, that *water* is eſſential to the formation of inflammable air. In all the liquid ſubſtances mentioned above, the water that enters into their compoſition is ſufficient for the purpoſe; and ſpirit of wine, and æther, appear to contain more water than is neceſſary. But when the ſubſtances are *dry*, and water does not enter as a neceſſary ingredient into their compoſition, water muſt be introduced into the proceſs. This is the caſe with all the *metals*, and it is no leſs ſo with *ſulphur*, *arſenic*, and probably other ſubſtances of a ſimilar nature, which mere heat only ſublimes.

Tranſmitting ſteam over a quantity of *ſulphur,* which was melting in a hot earthen tube, I procured from it a conſiderable quantity of inflammable air, without any fixed air; and by analyſis it appeared to be of the ſame quality with that which is procured from iron by oil of vitriol. This proceſs is rather troubleſome, on account of the ſulphur ſubliming, and filling up the tubes through which the air is conveyed.

I then repeated the ſame proceſs with *arſenic,* and from this ſubſtance alſo I procured air in great plenty. One-ſeventh of it was fixed air, but the reſt ſtrongly inflammable, and the ſmell of it could not be diſtinguiſhed from that of phoſphorus. Twenty ounce meaſures of this air weighed 4½ grains leſs than an equal quantity of common air. This experiment was no leſs troubleſome than the preceding, on account of the arſenic ſubliming, and choking up the tubes.

Having found a very heavy kind of inflammable air by heating the *ſcales of iron* mixed with *charcoal*, I made the following experiment in order to aſcertain the quantity of air that might be procured from a given quantity of theſe materials. Mixing two ounces of the *ſcales*, or *finery cinder* (which I found to be the ſame thing) with one ounce of perfect charcoal, I got from it, in an earthen retort, 580 ounce meaſures of air, one-tenth of the firſt part of which was fixed air; but afterwards it was all inflammable. The ſubſtances were pretty firmly concreted together, and weighed 1044 grains; ſo that the loſs of weight was 396 grains, which muſt have been very nearly the weight of the air procured. Forty ounce meaſures of this air, freed from all fixed air, weighed two grains more than an equal quantity of common air.

Beſides the *water*, which ſeems to be eſſential to the conſtitution of inflammable air, this ſpecies of air readily imbibes more water, which adds greatly to its ſpecific gravity. This ſeems at leaſt to be indicated by the following experiment. Filling a dry bladder with inflammable air, received immediately from the veſſel containing the iron and diluted oil of vitriol, from which it was generated, I found that 30 ounce meaſures of it weighed more than ſeventeen grains leſs than an equal bulk of common air; but when I weighed that inflammable air of the ſame kind which had been confined by water in the ſame bladder, it was only fourteen grains leſs than an equal quantity of common air. This I repeated ſeveral times with the ſame reſult. This air, therefore, could only be about three times lighter than common air; whereas the other was more than ten times lighter.

Having frequently examined the ſpecific gravity of inflammable air which has been long confined by water, by weighing it in a bladder, and then preſſing out the air, and weighing it when empty (which has the ſame effect as weighing it full of common air) I have ſeldom found ſuch air more than five times lighter than an equal portion of common air; ſo that the ſpecific gravity of this air is ſoon doubled by being kept in theſe circumſtances.

SECTION VI.

Of ſulphurated inflammable Air.

THERE is no kind of air which admits ſuch a variety of modifications as the inflammable; nor ſhall we think this extraordinary, when we conſider that phlogiſton, which is the diſtinguiſhing ingredient in it, enters into a greater variety of combinations with *ſolid ſubſtances* than perhaps any other principle in nature, and is the cauſe of a greater variety of properties in them. Spirit of wine, oil, ſulphur, charcoal, and metals, are ſubſtances as different from each other, both in their external appearance, their degrees of conſiſtence, and other chemical properties, as any things in nature, and yet the principal ingredient in them all is the ſame phlogiſton, as may be proved by the actual transferring of it from any one to any other of them. Inflammable air likewiſe extracted from each of theſe ſubſtances, as alſo that from putrid vegetables, and by other proceſſes, of which an account has been given in the preceding ſection, are all remarkably different, and appear to be ſo, as we ſhall preſently ſee, when they are decompoſed. I ſhall now give an account of another ſpecies of this kind of air, which I term *ſulphurated*, from the ſtrong ſmell that it has of ſulphur, or rather liver of ſulphur, and its being loaded with a greater quantity of matter, which, though at the firſt black, yet on expoſure to the air preſently aſſumes a yellowiſh colour. I ſhall recite the experiments in which I obſerved this peculiar ſpecies of air, in the order in which I made them, noting the other appearances that accompanied them, though they have not any immediate relation to the *air* of which I am treating.

When I was engaged in that courſe of experiments in which ſteam, and the vapour of various fluid ſubſtances, was brought into contact with ſolid ſubſtances red-hot, I treated *mangane/e* in this manner, and eſpecially a quantity with which Mr. WOULFE had formerly furniſhed me, which was not in powder, but in a large maſs, juſt as it is dug out of the earth. A few ounces of this I put into an earthen tube, open at both ends. But cloſing one of them with a cork, while the middle part of the tube was red-hot, and the other orifice was furniſhed with an apparatus proper for collecting the air that might be expelled from it, I received 40 ounce meaſures of air, of which one-ſixth was fixed air, and the reſt of the ſtandard of 1.7, lambently inflammable. No more air coming in this diſpoſition of the apparatus, I opened the other end of the tube, and with a proper contrivance for the purpoſe, ſent through it a quantity of ſteam; in which circumſtance air was produced more copiouſly than before. Of this I received about 50 ounce meaſures, obſerving that one-ſeventh of it was fixed air, and the reſt of the ſtandard of 1.8, not lambently, but exploſively inflammable. The laſt portions of this air were very turbid, and the ſmell of the air, and eſpecially that of the laſt portion, was very ſulphureous, and, I obſerved, tinged the water of a very dark colour, by depoſiting in it a quantity of blackiſh matter. However, the air itſelf became preſently tranſparent, and had no other appearance than that of any other kind of air, when I left in my trough a jar filled with it.

Having been intent on ſome other experiments, I was ſurpriſed to find, on looking on the jar about ten minutes afterwards, that it was quite black, ſo that I could ſee nothing in the inſide of it. In order to obſerve how it came to be ſo, I afterwards filled another jar with this kind of air, and obſerved that when the water was well ſubſided, black ſpecks began to appear in different places, and, extending themſelves in all directions, at length joined each other, till the whole jar was perfectly black, and the glaſs quite opake. When this was done, I transferred the air to another clean jar, and it ſoon produced the ſame effect upon this, though it never became ſo black as the jar in which it had been firſt received. It alſo frequently happened that only the lower part of the jar would become black, as if this matter, with which it was loaded, had kept ſubſiding, though inviſibly, in the maſs of air, and occupied the lower regions of it only, leaving the upper part entirely free from it. When the veſſels thus tinged black were expoſed to the open air, that colour preſently diſappeared, and a yellow or brown incruſtation was left upon it.

Thinking, from this circumſtance, that this black coating conſiſted of ſome volatile phlogiſtic matter, I placed the jars which had this black tinge with their mouths inverted in veſſels of water, in order to obſerve the effect which the change of colour might have on the common air contained in them. In theſe circumſtances the black tinge preſently went off, and was ſucceeded by the yellow colour, but without producing any ſenſible change in the air. In ſome caſes, however, I thought that it was injured; but it was by no means ſo much ſo as I had expected. After depoſiting this black matter, the air ſtill retained its ſulphureous ſmell, and as far as I can judge, will never entirely leave it.

It is by no means the univerſal property of manganeſe to yield this ſulphurated inflammable air, but muſt have been owing to ſomething peculiar to this ſpecimen, and perhaps to ſomething accidentally mixed with it. For when I repeated the experiment with other manganeſe, which I had from a glaſs-houſe, in which it is uſed, I had no ſuch appearance. From four ounces of this manganeſe, treated as the preceding, I got without ſteam, 256 ounce meaſures of air, of which about one-tenth was fixed air. Then ſending ſteam over it, I got more air, but in no great quantity, about ten ounce meaſures in an hour; though probably much more might have been procured, if the proceſs had been continued. This was dephlogiſticated; for, mixed with two equal meaſures of nitrous air, the ſtandard was

0.28, which ſhews that it was exceedingly pure. But one-tenth of this was fixed air, as in the former portion, which agrees with the experiments I formerly made with this ſubſtance when I found that heat alone would expel from it a quantity of very pure air.

The next time that I got this ſulphurated inflammable air, was as unexpected as the preceding; and this experiment was the firſt thing that gave me any inſight into the nature of it. Having occaſion to make a large quantity of inflammable air, inſtead of freſh burnings of iron, I happened to take ſome, parts of which had been heated by a burning lens in vitriolic acid air, in which, as I have obſerved, it melts with great readineſs, and gathers into balls. When this iron was diſſolved in diluted oil of vitriol, though there were only a few pieces in the quantity that I uſed which had been melted in this manner, the water in which the air was received was very black, and depoſited more ſediment than in the experiment with the manganeſe. The jars alſo which contained it were preſently black as ink, but became yellow when expoſed to the open air. This inflammable air had alſo the ſame offenſive ſulphureous ſmell; ſo that there could be no doubt of its being the ſame kind of air which I had got from Mr. Woulfe's manganeſe. There was in it, however, a mixture of vitriolic acid air, as I perceived when I burned a large quantity of it in a glaſs balloon, in order to collect the *water* that might be produced in this proceſs. All the inſide of the balloon was filled with a denſe white cloud, all the time that the air was burning in it, and the water produced was very ſenſibly acid. In reality, the ſame effects were produced as if ſulphur had been burned in the veſſel.

As I had no doubt, but that the iron which had been melted in vitriolic acid air was the ſame as what is called *ſulphurated iron*, or iron with which ſulphur is incorporated, I now completely aſcertained it by making a quantity of ſulphurated iron, dipping it when red-hot into melted ſulphur. This iron, treated as the other had been, yielded exactly ſuch air as I have been deſcribing, ſo that I could have no doubt with reſpect to the real origin of it.

When I decompoſed this air, by firing it with an equal quantity of dephlogiſticated air, the diminution of bulk was the ſame as when I uſed the common inflammable air, ſo that it did not appear to contain either more or leſs phlogiſton; but there was a ſmall quantity of *fixed air* produced, which is never the caſe with inflammable air procured with oil of vitriol, though it is when it is procured from iron with ſpirit of ſalt.

When the ſulphurated inflammable air is received in veſſels containing mercury, there is very little black matter depoſited from it, but it appears when it is transferred into veſſels containing water.

Though jars thinly coated with this black matter become yellow when expoſed to the open air, this is not the caſe with that which is collected from the water in which the air has been confined. For when the water is evaporated from it, it adheres to the evaporating veſſel in the form of a perfectly black incruſtation. This ſubſtance, though it does not burn blue on a hot iron, yet ſhews evident ſigns of containing ſulphur. For when the nitrous acid has taken from it its ſuperfluous phlogiſton, it has both the colour and the ſmell of ſulphur.

SECTION VII.

The Analyſis of different Kinds of inflammable Air.

BEFORE I proceed to the analyſis of the different kinds of inflammable air, which is the ſubject of this ſection, I ſhall obſerve, that the pureſt we can procure (which is that from metals by ſolution in the mineral acids, or rather that by means of ſteam from red-hot iron) ſeems to conſiſt of phlogiſton and water, and that neither *acid* nor *alkali* is a neceſſary ingredient in it; though, when it is produced by means of either of them, a ſmall portion of either may be retained in it, as an extraneous ſubſtance. That this, however, is the caſe, has been very clearly ſhewn by Mr. SENEBIER, though I think that the production of inflammable air by means of iron and ſteam only, without either acid or alkali, ſufficiently proves that his hypotheſis of inflammable air neceſſarily requiring ſome ſaline baſis cannot be well founded.

It was, indeed, my own firſt opinion, that inflammable air conſiſts of acid and phlogiſton. Afterwards I adopted the opinion of Mr. KIRWAN, viz. that it is pure phlogiſton in the form of air, but at preſent I am fully ſatisfied with the opinion of Mr. CAVENDISH, that *water* is an eſſential ingredient in the conſtitution of this kind of air. It may, indeed, appear extraordinary that ſince, according to the hypotheſis which at preſent ſeems to be well eſtabliſhed, water itſelf conſiſts of phlogiſton and dephlogiſticated air, this water ſhould exiſt in inflammable air as an eſſential part of it, without being reſolved into its conſtituent principles. But we are as yet ignorant of the principles of combination and coheſion in the parts which compoſe natural ſubſtances, much more than we are with reſpect to the conſtituent parts themſelves; though it is very poſſible that when we ſhall have diſcovered the latter (in which great progreſs is making every day) the knowledge of this will be a means of throwing ſome light upon the former; ſo that we may be able to tell why any particular ſubſtance is ſolid or fluid, why it exiſts in the form of elaſtic air, why metals are of a firmer texture than ſtones or ſulphur, or why ſome of the metals ſhould be ſo much harder than others, concerning which the moſt knowing among us cannot at preſent pretend to form even a plauſible conjecture. All that we can ſay is, that no mere *difficulty of conceiving* how a thing can be, ſhould be admitted as an argument againſt any concluſion fairly deduced from proper premiſes.

That no acid is neceſſarily contained, or at leaſt in any ſenſible quantity, either in inflammable air, though produced by means of acids, or in the dephlogiſticated air of the atmoſphere, is evident from the following experiment, which I made with the greateſt care. Taking a baſon which contained a ſmall quantity of water tinged blue with the juice of tumſole, I placed in it a bent tube of glaſs, which came from a veſſel containing iron and diluted oil of vitriol; and lighting the current of inflammable air, as it iſſued from this tube, ſo that it burned exactly like a candle, I placed over it an inverted glaſs jar; ſo that the mouth of it was plunged in the liquor. Under this jar the inflammable air burned as long as it could, and when it was extinguiſhed, for want of more pure air, I ſuffered the liquor to riſe as high as it could within the jar, that it might imbibe whatever ſhould be depoſited from the decompoſition of either of the two kinds of air. I then took off the jar, changed the air in it, and lighting the ſtream of inflammable air, replaced the jar as before. This I did till I had decompoſed a very great quantity of the two kinds of air, without perceiving the leaſt change in the colour of the liquor, which muſt have been the caſe if any acid had entered as a neceſſary conſtituent part into either of the two kinds of air. I alſo found no acid whatever in the water which was procured by keeping a ſtream of inflammable air conſtantly burning in a large glaſs balloon, through which the air could circulate, ſo that the flame did not go out. Neither was there any acid produced in the decompoſition of inflammable and dephlogiſticated air in a ſtrong cloſe glaſs veſſel.

With reſpect to inflammable air itſelf, I have before obſerved, that when ſufficient care is taken to free it from any acid vapour that may be accidentally contained in it, it is not in the ſmalleſt degree affected by a mixture of alkaline air. On the whole, therefore, I have at preſent no doubt but that pure inflammable air, though it certainly contains *water*, does not neceſſarily contain any acid. Yet an acid vapour may be eaſily diffuſed through it, and may perhaps in many caſes be obſtinately retained by it, as no kind of air ſeems to be capable of ſo great a variety of impregnations as inflammable air is.

That there are different kinds of inflammable air, has been obſerved by moſt perſons who have made any experiments on air. That which has been moſt commonly obſerved is, that ſome of them burn with what may be called a *lambent flame*, ſometimes blue, ſometimes yellow, and ſometimes white, like the flame from wood or coals in a common fire; whereas another kind always burns with an *exploſion*, making more or leſs of a report, when a lighted candle is dipped into a jar filled with it. Of the latter kind is that which is extracted from metals by means of acids, &c. and of the former kind is that which is expelled from wood, coal, and other ſubſtances by heat. It has alſo been obſerved, that theſe kinds of inflammable air have different ſpecific gravities, the pureſt kind, or that which is extracted from iron, &c. being about ten times lighter than common air, but ſome of the other kinds not more than twice as light.

The cauſe of this difference I once thought I had diſcovered to be the heavier kinds of inflammable air, containing a proportion of *fixed* air, ſo intimately combined with them, ſo as not to be diſcoverable by lime water, while the lighteſt kind contained no fixed air at all. This hypotheſis I formed from decompoſing them with common or dephlogiſticated air, by the electric exploſion. For, after the experiment with the heavier kinds of inflammable air, I always found a quantity of fixed air in the reſiduum, but none at all after the experiment with the lighteſt kind.

In order to decompoſe any kind of inflammable air, I generally mix it with an equal quantity of dephlogiſticated air, and then confine them in a ſtrong glaſs veſſel, previouſly filled either with water or mercury, and I make an electric ſpark in ſome part of the mixture, by means of wires inſerted through the ſides of the veſſel, and nearly meeting within it. By this proceſs, I imagined, that I was able to aſcertain two things relating to the conſtitution of different kinds of inflammable air, viz. the quantity of *combined fixed air* (as I then thought it to be) and likewiſe the relative quantity of phlogiſton contained in each of them. The former appeared by waſhing the air with lime water after the exploſion, and obſerving how much of them was abſorbed, and the latter by examining the reſiduum with the teſt of nitrous air, and obſerving the purity of it. In moſt of theſe experiments I made uſe of dephlogiſticated air, in preference to common air, becauſe I could not make ſome kinds of inflammable air to explode at all with common air. Otherwiſe I ſhould have preferred this, as being the moſt nearly of the ſame quality. However, I always noted the degree of purity of the dephlogiſticated air that I made uſe of before I began any of theſe analyſes.

Finding, however, that in ſome caſes more fixed air was found after the exploſion of the two kinds of air, than could poſſibly have been *contained* in the inflammable air, on account of the weight of it, I was ſatiſfied that there muſt have been a real *generation* of it, by an union of the inflammable and dephlogiſticated air. It is remarkable, however, that ſome kinds of inflammable air ſhould ſo readily unite with dephlogiſticated air, ſo as to make a conſiderable quantity of fixed air; and that others, treated in the ſame manner, ſhould not do this at all; and alſo that thoſe which do it ſhould be the heavier kinds of inflammable air. This is a new and curious ſubject of inveſtigation.

The pureſt and lighteſt inflammable air is that which is extracted from iron, and other metals, by a ſolution in the acids, or by means of ſteam. One meaſure of this kind of air, and one of dephlogiſticated (ſuch as that, when mixed with two equal quantities of nitrous air, there remained 0.72 of a meaſure) exploded together in the manner deſcribed above, were reduced to 0.6 of a meaſure, no fixed air was found in the reſiduum, and when examined with an equal quantity of nitrous air, was reduced to 0.87 of a meaſure.

With the ſame dephlogiſticated air I examined inflammable air that had been got from a mixture of finery cinder and charcoal, and found, that after the exploſion, the two meaſures were reduced only to 1.85, but that by waſhing in the lime water, they were reduced to 1.2. Conſequently 0.65 of a meaſure of fixed air had been generated in the proceſs. When this was ſeparated from it, and the remainder examined by nitrous air, it appeared to be of the ſtandard of 0.9; ſo that the dephlogiſticated air had been more injured by this than by an equal quantity of the common inflammable air, though the difference in this reſpect was not conſiderable.

In another proceſs with this kind of inflammable air, the diminution after the exploſion was to 1.55, and that after the waſhing in lime water to 0.65 of a meaſure; ſo that there had been a generation of 0.9 of a meaſure of fixed air. In another experiment the firſt diminution was to 1.6, and the ſecond 0.66, ſo that 0.94 of a meaſure of fixed air had been produced. And laſtly, in another proceſs, the firſt diminution was to 1.6, and the ſecond to 0.6 of a meaſure; ſo that there was a generation of one complete meaſure of fixed air, and this was a clear proof that it could not have been *contained* in a combined ſtate, as I at firſt imagined, in the inflammable air; ſince then it muſt have been much heavier than I had ever found it to be; for though I found the ſpecific gravity of it to be ſomething different at different times (and the preceding experiments were made with the air of different proceſſes) I had never found that 40 ounces meaſure of this air was more than 2 grains heavier than an equal bulk of common air.

This, indeed, is a remarkable circumſtance with reſpect to a ſpecies of inflammable air, as it does not appear by the teſt of lime water to contain any fixed air; but it ought to have weighed more than one-half heavier than common air, to have actually contained in combination all the fixed air that I found after its exploſion with the dephlogiſticated air. Indeed, if any quantity of inflammable air, of about the ſame ſpecific gravity with common air (which is the caſe with that ſpecies of it which I am now conſidering) yield ſo much as ſeven-tenths of its bulk of fixed air, in conſequence of its exploſion with dephlogiſticated air, it is a proof that at leaſt part of that fixed air was generated in the proceſs, becauſe ſeven-tenths of ſuch fixed air would weigh more than the whole meaſure of the inflammable air.

Inflammable air from *ſpirit of wine* (made by tranſmitting it *in vapour* through a red hot earthen tube) being analyſed in the manner above mentioned, one meaſure of it, and one of the ſame dephogiſticated air that was uſed in the former experiment, were reduced to one meaſure, and by waſhing in lime water to 0.6 of a meaſure; ſo that four-tenths of its bulk of fixed air had been generated in the proceſs. The ſtandard of the reſiduum was 1.7; ſo that the dephlogiſticated air had been injured much more than in either of the former proceſſes, and conſequently it muſt have contained more phlogiſton.

I found conſiderable variations in the experiments with this, as well as with ſome other kinds of inflammable air. For, in another proceſs, in which the earthen tube had been filled with bits of crucibles (in order to expoſe more red ſurface to the vapour of the ſpirit of wine) the firſt diminution was to 1.6, the ſecond to 1.4; and the ſtandard of the reſiduum was 1.84. In another proceſs with this kind of air, the firſt diminution was to 1.2, and the ſecond to 0.9.

Having procured a quantity of inflammable air, by tranſmitting ſteam over red hot *platina*, I analyſed it in the ſame manner, and found that the two meaſures were reduced by the exploſion to 0.72. It contained no fixed air, and the reſiduum was of the ſtandard of 0.9.

Inflammable air, procured by making ſteam paſs over melted brimſtone, being examined in the ſame manner, the firſt diminution was to 0.6, and no fixed air was found in it. In this reſpect it ſeems to have been the ſame thing with inflammable air from iron, but the ſtandard of the reſiduum was 0.95; ſo that it ſeems to have contained more phlogiſton. But I have found the ſtandard of this reſiduum ſubject to conſiderable variations, ſuch as I could not well account for.

Inflammable air procured in the ſame manner from melted *arſenic*, appeared to be very different from that which was extracted from brimſtone. For the two meaſures were reduced by the exploſion to 1.15, and by waſhing with lime water, to 0.95; ſo that one fifth of its bulk of fixed air had been generated. The ſtandard of the reſiduum was 0.82.

At the ſame time I examined ſome inflammable air, that had been made by heating bits of crucibles in *alkaline air*, and found that the two meaſures were reduced by the exploſion to 0.96 of a meaſure, that the reſiduum contained no fixed air, and was of the ſtandard of 0.8.

The inflammable air that is made from *æther*, by tranſmitting the vapour of it through a red hot earthen tube, very much reſembles that which is got from ſpirit of wine. The two meaſures were reduced by the exploſion to 1.36, and by waſhing in water to 1.2; ſo that 0.16 of a meaſure of fixed air had been generated, and the reſiduum was of the ſtandard of 1.9.

Inflammable air procured by tranſmitting ſteam over red hot *charcoal of metals*, in the ſame manner as it is got from other charcoal, produces a conſiderable quantity of fixed air. For when the experiment was made with this air, the firſt diminution was to 1.12, and the ſecond to 0.8; ſo that 0.32 of a meaſure of fixed air was generated, and the ſtandard of the reſiduum was 1.9. This analyſis was of the firſt portion that came in the proceſs. The ſecond was ſomething different. For with this, the firſt diminution was to 1.0, and the ſecond to 0.75, the reſiduum being the ſame as before, viz. 1.9. Thirty ounce meaſures of this air weighed 8 grains leſs than an equal bulk of common air.

Analyzing the inflammable air from *coak*, or the charcoal of pitcoal produced by ſteam, the firſt diminution was to 1.15, and the ſecond to 0.95; ſo that one fifth of its bulk of fixed air was generated. The ſtandard of the reſiduum was 1.9. But I muſt obſerve, that the dephlogiſticated air with which this experiment was made, was ſo impure as hardly to deſerve the name. For two meaſures of nitrous air and one of this, occupied the ſpace of two meaſures. But this circumſtance may not affect the quantity of fixed air generated in the proceſs. Thirty ounce meaſures of this air weighed ten grains leſs than an equal bulk of common air.

Analyzing the inflammable air that was produced in the ſame manner from *ſpirit of turpentine*, the firſt diminution was to 1.7, and the ſecond to 1.6; ſo that only one tenth of fixed air was produced. The reſiduum was of the ſtandard of 1.9. Thirty ounce meaſures of this air weighed eight grains leſs than an equal bulk of common air.

When the vapour of ſpirit of wine was made to paſs over melted metals, inflammable air was produced from it juſt as if no metals had been concerned; but when I examined the air that was procured in this manner, it did not appear to be quite the ſame with that which came from pure ſpirit of wine. Analyzing the air that was produced in a proceſs with *copper*, the firſt diminution was to 1.7, the ſecond to 1.56, and the ſtandard of the reſiduum was 1.78. Thirty ounce meaſures of this air weighed ſeven grains leſs than an equal bulk of common air.

In another proceſs with inflammable air, procured in this manner, the firſt diminution was to 1.55, the ſecond to 1.48, and the reſiduum was of the ſtandard of 1.86. Thirty ounce meaſures of this air weighed 8½ grains leſs than an equal bulk of common air. This air, it is obſervable, produced much leſs fixed air than the other, and it was alſo ſpecifically lighter than it.

When this proceſs was made with the air procured by tranſmitting vapour of ſpirit of wine over melted *ſilver*, the firſt diminution was to 1.9, the ſecond to 1.78, and the ſtandard of the reſiduum was 1.9. Thirty ounce meaſures of this air weighed eight grains leſs than an equal bulk of common air.

In the analyſis of the air procured by this proceſs from *lead*, the firſt diminution was to 1.78, the ſecond to 1.6, and the ſtandard of the reſiduum was 1.78.

I examined, at the ſame time, inflammable air procured from *bones*, and alſo from *charcoal*, viz. by tranſmitting ſteam over them when they were red hot in earthen tubes, when all air had been previouſly expelled from them by heat. With the former, the firſt diminution was to 0.67, and the ſecond to 0.58; ſo that the fixed air produced was extremely inconſiderable, viz. only 0.09 of an ounce meaſure. The ſtandard of the reſiduum was 1.47. In the experiment with the air from *charcoal*, the firſt diminution was to 1.5, and the ſecond to 0.74; ſo that the fixed air was 0.76, and the ſtandard of the reſiduum was 1.7. From this experiment it may be inferred, as mentioned before, that inflammable air from *bones*, is a kind of medium between that from *metals*, and that from *charcoal.* In another proceſs with air from charcoal, the firſt diminution was to 0.82, and the ſecond to 0.63, and the ſtandard of the reſiduum was 1.37.

I made the following experiment to aſcertain how much phlogiſton is contained in inflammable air from charcoal. In five ounce meaſures of this kind of air, I revived lead from maſſicot till it was reduced to three fourths of an ounce meaſure, when the lead revived weighed ten grains, and there remained one ounce meaſure of fixed air. But the minium itſelf yielded a little fixed air.

It is obſervable, that when wood is heated in an earthen retort, the firſt air that comes over is conſiderably different from that which comes in the middle, or at the end of the proceſs. Indeed the properties of it are continually changing during the whole proceſs. The firſt portion burns with a lambent white flame, like that from burning wood in an open fire; afterwards the flame is blue, and towards the end of the proceſs it is conſiderably exploſive, almoſt like air from iron. Alſo the air that comes over firſt is very turbid, owing perhaps, to oily, and other matters, that are rendered volatile by heat.

Having procured air from dry beech-wood, I examined, in the method deſcribed above, the firſt portion of it, and alſo one of the middle ones. The former I found to contain 4½ tenths of its bulk of uncombined fixed air, the ſecond portion only two tenths. Afterwards it is well known that air procured in this manner ceaſes to have any uncombined fixed air in it.

When I examined the firſt portion of air, after the uncombined fixed air had been ſeparated from it, the firſt diminution was to 1.36, and the ſecond to 0.9; ſo that 0.46 of a meaſure of fixed air was generated, and the ſtandard of the reſiduum was 1.9.

When the ſecond portion was examined, after the uncombined fixed air was likewiſe ſeparated from it, the firſt diminution was to 1.66, and the ſecond to 1.46; ſo that the fixed air generated in the proceſs, was 0.2 of a meaſure, that is, leſs than in the former experiment, and in nearly the ſame proportion as the uncombined fixed air had been. The ſtandard of the reſiduum in this laſt caſe was 1.15. At the ſame time repeating the experiment with the ſame dephlogiſticated air and inflammable air from iron, the diminution after the exploſion was to 0.55, and the ſtandard of the reſiduum was 1.48, which is the uſual reſult of the decompoſition of inflammable and dephlogiſticated air, when both of them are as pure as they are generally procured.

There was a great quantity of fixed air produced by the decompoſition of ſome inflammable air extracted from ſome rich *mold* in a gun-barrel, which Mr. YOUNG was ſo obliging as to ſend me. It burned with a lambent blue flame, and had a peculiarly offenſive ſmell, the ſame, as he obſerves to me, that is yielded by air procured from putrid vegetables. Of this air one twentieth part is uncombined fixed air. When this was ſeparated from it, and the remainder decompoſed with dephlogiſticated air, the firſt diminution was to 1.4, and the ſecond to 0.67; ſo that there was a generation of 0.73 of a meaſure of fixed air. The reſiduum was of the ſtandard of 0.6.

The inflammable air that is procured from *caſt iron* has a peculiarly offenſive ſmell. On this account I had imagined that it might contain more phlogiſton than common inflammable air, ſo as to abſorb more dephlogiſticated air than the other. But this did not appear to be the fact. For when I mixed one meaſure of each of the kinds of inflammable air with four meaſures of common air, the diminution after the exploſion, was the very ſame with both, viz. to 1.56.

Though I think it to be unqueſtionable, from the preceding experiments, that part at leaſt of the fixed air which is found on the decompoſition of lambent inflammable air is *generated* in the proceſs; yet, in another experiment that I made, it ſhould ſeem that fixed air, or the elements, as we may ſay, of fixed air, may enter into the compoſition of inflammable air, and actually remain there, without being diſcoverable by lime-water.

I took a quantity of *ſlaked lime*, which had been long kept cloſe corked in a bottle, and found that when it was heated in an earthen retort, it gave air, of which one fifth was for the moſt part fixed air; but in the gun-barrel the ſame lime yielded no fixed air at all, but a great quantity of pure inflammable air, of the exploſive kind, like that which is got from iron alone with water. That the water in ſlaked lime will enable the iron of the gun-barrel to yield inflammable air cannot be queſtioned, but then what became of the fixed air which the ſame lime would have yielded in an earthen retort?

This experiment appearing rather extraordinary, I repeated it, with all the attention I could give to it, and had the following reſult. I heated three ounces of ſlaked lime (but which had been ſome time expoſed to the open air) in an earthen tube, and got from it 14 ounce meaſures of air, of which only 2½ meaſures remained unabſorbed by water, all the reſt being fixed air. This reſiduum was ſlightly inflammable, but not perfectly phlogiſticated. For, examining it with the teſt of nitrous air, the ſtandard of it was 1.6.

Immediately after this I heated another three ounces of the ſame ſlaked lime in a gun barrel, and got from it about 20 ounce meaſures of air, of which no part was fixed air, but all inflammable. I expected, however, to have found fixed air on the decompoſition of this inflammable air with dephlogiſticated air; but after this proceſs it appeared to be exactly ſuch inflammable air as is procured from metals by the mineral acids, or rather by ſteam. For the diminution of the two kinds of air was the ſame, and though there was ſome appearance of fixed air in the reſiduum, it was not ſo much as is found after the decompoſition of the inflammable air that is procured by means of ſpirit of ſalt. In this caſe, therefore, there are no leſs than 11½ ounce meaſures of fixed air abſolutely unaccounted for, unleſs it be ſuppoſed that it was reſolved into its conſtituent principles, phlogiſton or dephlogiſticated air, and that the latter was decompoſed as it was produced. This, therefore, I think muſt have been the caſe.

Thinking that the two kinds of air might incorporate when one of them was generated within the other, I filled a gun barrel previouſly full of mercury with fixed air, and put the cloſed part of it into a hot fire. Inflammable air was accordingly produced, but when the fixed air was ſeparated from it, it exploded juſt like inflammable air from iron only.

I made an experiment ſomething ſimilar to this, by heating iron turnings in five ounce meaſures of fixed air, when the quantity of it was increaſed about one ounce meaſure, and there remained 1 and ¾ of an ounce meaſure unabſorbed by water. This was inflammable,

flammable, and burned with a lambent blue flame, not like inflammable air from iron. It ſhould ſeem, therefore, that in this experiment, $\frac{3}{4}$ of an ounce meaſure of inflammable air had been formed by the union of the fixed air with the phlogiſton from the iron. This experiment I repeated with the ſame reſult, and I farther obſerved, that though the inflammable air procured in this manner did not appear, by the teſt of lime water, to contain any fixed air, yet when it was decompoſed, by being fired together with an equal quantity of dephlogiſticated air, fixed air was found in the reſiduum. For the firſt diminution was to 1.45, and one-third of this reſiduum was fixed air. From this fact it ſhould ſeem that, though in ſome caſes, fixed air muſt be *generated* by the decompoſition of inflammable and dephlogiſticated air, yet that inflammable air, when thus produced in contact with fixed air, may *combine* with it, ſo as to be properly *contained* in it, and in ſuch a manner, as that it cannot be diſcovered by lime water.

I alſo obſerved, after Mr. METHERIE, that though no fixed air be found on the decompoſition of dephlogiſticated air with inflammable air procured by means of oil of vitriol, a ſmall quantity is produced when the inflammable air is procured by means of ſpirit of ſalt. I did not find, however, more than a fortieth part of the reſiduum to be fixed air, when I decompoſed equal quantities of the two kinds of air.

I found ſome appearance of fixed air, but even leſs than in the laſt-mentioned experiment, when I heated red precipitate in inflammable air procured by means of ſpirit of ſalt. For there was a ſlight turbid appearance in the lime water to which it was admitted, but I could not meaſure any certain diminution of bulk from agitation, as in the other caſe.

SECTION VIII.

The Analyſis of Alkaline Air.

THAT alkaline air contains phlogiſton appears both by the calces of metals being revived in it, and by its being converted into a ſpecies of inflammable air, by the electric exploſion, and other proceſſes. By the following experiments it ſeems to contain leſs phlogiſton than an equal bulk of inflammable air from iron, and about one-fourth of its bulk of phlogiſticated air. But it is not poſſible to aſcertain this with perfect exactneſs, from the nature of the proceſſes. I ſhall therefore recite the experiments in the order in which they were made, ſome of them being more, and others leſs accurate.

I firſt obſerved that ſome *maſſicot*, which I uſed in ſome of theſe experiments, contained a portion of fixed air, which could be expelled from it by heat; but when I melted it in alkaline air, by which means the lead was always revived, I found no fixed air in the reſiduum, but only pure phlogiſticated air. This, however, is eaſily accounted for, as the alkaline air would immediately unite with any fixed air that ſhould be ſet looſe in the proceſs.

With reſpect to the quantity of lead revived in alkaline air, it is difficult to be aſcertained, on two accounts. In the firſt place, ſome of the calx is blackened, and imperfectly revived; and again, if particular care be not taken, the lead that is revived will be diſſolved in the mercury, by which the air muſt neceſſarily be confined. To prevent this laſt inconvenience, I put the powdered maſſicot into ſmall earthen cups, which I contrived to place with their mouths upwards, ſo that when the lead was revived, by means of a burning lens, it would remain in the cup, and not mix with the mercury by which it was ſupported. On theſe accounts, however, the largeſt quantity of lead that was revived muſt always be conſidered as neareſt the truth.

In three ounce meaſures of alkaline air, I firſt revived ſix grains of lead, then in 3½ 16 grains, in 2½ 13 grains, and in 3¾ 12 grains. In five ounce meaſures I revived 10 grains, beſides blackening a quantity of it, and there remained one ounce meaſure of phlogiſticated air. But the experiment which I made with the moſt care, and on which I place the greateſt dependence, was the following.

In 7½ ounce meaſures of alkaline air, I revived 26½ grains of lead, and there remained 2½ ounce meaſures of phlogiſticated air. In this proportion 100 ounce meaſures of alkaline air would revive 352 grains of lead; whereas 100 ounce meaſures of inflammable air from iron would revive 480 grains of lead. But alkaline air is reſolvable into conſiderably more than twice its bulk of inflammable air, and therefore it might have been expected that it would have revived much more lead. Would it not, therefore follow from this, that phlogiſticated air contains a very great quantity of phlogiſton, many times more than an equal bulk of pure inflammable air. Suppoſing alkaline air to be reſolvable into no more than twice its bulk of inflammable air, a hundred ounce meaſures of it ought to revive 960 grains of lead. But in the proportion mentioned above, it would revive no more than 608 grains. As much phlogiſton, therefore, as would revive 342 grains of lead (which is the difference between theſe two numbers) ſhould be contained in about 25 ounce meaſures of phlogiſticated air. Conſequently 100 ounce meaſures of phlogiſticated air would revive 1368 grains of lead, which is more than twice as much as an equal quantity of pure inflammable air would revive. It muſt, however, be conſidered, that the inflammable air, into which alkaline air is reſolvable, is not the ſame with that which is procured from iron, and may contain much leſs phlogiſton.

To theſe experiments, on the revival of lead in alkaline air, I ſhall ſubjoin an account of ſome that were made with other calces, heating them all, when confined by this air, by means of a burning lens.

Glaſs of antimony abſorbed a part of the alkaline air, and there was the appearance of metal in ſeveral ſmall globules, whereas I could get no ſuch appearance from the ſame ſubſtance in inflammable air.

By heating ſcales of iron in alkaline air, it was much increaſed in quantity, and then I found it to contain no fixed air, but the whole was ſtrongly inflammable. No water was produced in this proceſs. The iron was revived, being perfectly ſoluble in diluted oil of vitriol; and from being 24 grains had loſt one grain in weight.

As alkaline air evidently contains phlogiſton, and as ſuch was capable of reviving lead, and no doubt the other metals, I had the curioſity to make uſe of it in other circumſtances in which I had before uſed inflammable air; and among other things I tried the effect of heating red precipitate in it. That the mercury ſhould be revived in this proceſs is nothing extraordinary, becauſe it will be revived from the ſame ſubſtance in any kind of air, and even in *vacuo*, the heat applied to it expelling from it the dephlogiſticated air which it contains. But there is one circumſtance attending this experiment which I cannot ſatisfactorily account for. At the ſame time that the metal revives, a conſiderable quantity of water is produced, whereas little or none is found when the ſame ſubſtance is revived in inflammable air. It has even run down in drops in the inſide of a veſſel which contained about five ounces meaſures of the air. The air which remained after this proceſs was about half as much as the original quantity of alkaline air, and it had in it a quantity of undecompoſed dephlogiſticated air. For with two equal meaſures of nitrous air, the ſtandard of it was 1.4, and there was nothing inflammable in it.

Again, I threw the focus of the lens upon red precipitate, in alkaline air, till three meaſures of it were reduced to two. Water was produced in the proceſs, and the air that remained was conſiderably dephlogiſticated; the ſtandard of it, with a mixture of two equal meaſures of nitrous air, being 1.7.

But in another experiment of this kind, there muſt have been a quantity of inflammable air ſet looſe from the alkaline air, as well as deplogiſticated air from the red precipitate; becauſe, after the operation had continued ſome time, there was a violent exploſion within the veſſel; which threw it many feet perpendicularly into the open air, as I was holding it in my hands. In this experiment I had been particularly careful to make every thing concerned in it as dry as I poſſibly could, in order to ſatisfy myſelf with reſpect to the production of the *water* which I had found before; and there was time enough before the exploſion to obſerve that water was certainly produced in the proceſs. To appearance the quantity of air was never much diminiſhed.

That water ſhould be produced when the ſcales of iron are heated in alkaline air, which I found to be the caſe, is not extraordinary, as the ſame is found on heating this ſubſtance in inflammable air; and it appears to contain within itſelf the water that is expelled from it. Alſo though, in the experiment with red precipitate and alkaline air, neither of the ſubſtances contain the water; yet each of the *elements* of water are contained in them, viz. the dephlogiſticated air in the red precipitate, and the phlogiſton in the alkaline air. But the difficulty is how to account for the formation of water in this caſe, and the non-formation of it when inflammable air is uſed. The difference of circumſtances in the two caſes is, that the inflammable air is ready formed in the one caſe, whereas the elements of it only are contained in the alkaline air. But how this difference in the circumſtances ſhould occaſion this difference in the reſult I cannot tell; ſo that this, together with many other things, muſt remain to be inveſtigated by future experiments.

SECTION IX.

Of the Converſion of Alkaline Air into Inflammable Air.

I FORMERLY found that, by taking the electric ſpark in alkaline air, a very remarkable change was made in it, as from being a ſpecies of air moſt readily abſorbed by water, it became immiſcible in water, being proper inflammable air, and nearly three times its former bulk. I had then only obſerved the ſimple *fact*, but had made no progreſs in the inveſtigation of the *cauſe*. I have ſince made a few ſteps towards it, and have found that the change is produced by *heat*. But in what manner it operates to produce this effect remains to be enquired into.

When I reflected upon this change of the alkaline air by the electric exploſion, I conſidered that it might be produced either by *electricity as ſuch*, or by the *light*, or the *heat*, which accompany the experiment. In order to try the effect of *light*, I expoſed a ſmall quantity of alkaline air to a ſtrong light made by a burning lens, for ſeveral hours. But this made no ſenſible change in the quantity of it. I therefore, concluded that the proper agent in this proceſs was not light.

I did not immediately proceed to try the effect of heat, but obſerved it by accident ſome time afterwards, in the following manner. At the time that I was reviving metals in inflammable and alkaline air, I put a quantity of ochre into the latter, expecting to revive it in the form of iron; and in this I ſeemed to ſucceed, as the ochre became black in the courſe of the proceſs. But what ſtruck me the moſt was, that inſtead of perceiving the quantity of air to decreaſe, as I had expected, there was very ſoon a viſible increaſe of it. Examining the air, I found it to contain no fixed air, and to be all ſtrongly inflammable.

Still attending to other circumſtances beſides the change of the alkaline air, I proceeded to heat in it ſcales of iron, and iron itſelf, of different kinds, and ſtill found that the air was increaſed in quantity, and changed in its quality. But in this way I could never increaſe the bulk of the air more than twice, and ſeldom quite ſo much. Alſo, after the proceſs, when I could make no farther change in the air, I ſtill perceived a very ſtrong ſmell of volatile alkali. The iron had undergone no change that I could diſcover, ſo that I concluded that the air had been affected by means of *heat* only.

To aſcertain this, I then proceeded to heat in the alkaline air bits of dry crucibles, or of earthen retorts, which had been juſt before expoſed to very great heats, ſo that they could not be ſuppoſed to give out any air in this proceſs, and therefore could only ſerve to produce a ſtrong heat in the alkaline air. The reſult ſeemed to confirm my ſuppoſition. For in theſe experiments with earthy ſubſtances the effect was always the ſame as when I had made uſe of ochre or iron. The air was increaſed in bulk, but the quantity not quite doubled, and it was become inflammable.

The bits of *white* earthen ware having always become *black* in this proceſs, I ſuſpected that this kind of ſubſtance might have affected the air in ſome other way than by merely communicating heat to it. To try this, I uſed the ſame ſmall bit of crucible again and again, and finding the ſame effect from heating it at the laſt, as at the firſt, I could not help concluding that the change produced in the air had been effected by means of *heat* only.

In all theſe experiments, however, there was a ſtrong *light* as well as an intenſe heat concerned. In order to aſcertain, therefore, whether the light, at leaſt in conjunction with heat, might not have had ſome effect in this change of the air, I heated a quantity of it in a coated green glaſs retort. The mouth of the retort was plunged in a baſon of mercury, and I received in a glaſs tube filled with water all the air that was driven out of the retort by heat. At firſt it was all abſorbed by the water, being merely alkaline air expelled by rarefaction. But when I concluded that the bulb of the retort was become red-hot, the bubbles that were driven out were not wholly abſorbed by water, and by degrees no part of them was abſorbed. This air I examined, and found it to be inflammable. The retort was melted in the proceſs, ſo that I could not examine the air that remained in it; but my purpoſe was ſufficiently anſwered, as I ſaw no room to doubt, but that the change of the alkaline into inflammable air was produced by *mere heat*, without the aid of light, if a red heat can be ſo termed. How the heat operates in this caſe I cannot tell.

It was obſervable, that whenever I changed alkaline air into inflammable, by heating in it bits of crucibles or retorts, into the compoſition of which *clay* enters, they always became black. There was room, therefore, to ſuppoſe that it was not by means of *mere heat* that this change was made, but that ſomething might be depoſited from the air, which might attach itſelf to the clay. Indeed, if this was not the caſe, I do not ſee why the clay ſhould become black; though, perhaps, part of the ſame phlogiſton which forms the inflammable air may be attracted by the red-hot clay, without there being any proper decompoſition of the air.

That this is the caſe, ſeems probable from an experiment in which I uſed white *porcelain*, inſtead of bits of earthen ware, into the compoſition of which clay enters. For this ſubſtance did not become black in the proceſs, and yet inflammable air was produced. But I muſt obſerve, that a conſiderable *time* was requiſite to produce this effect in theſe circumſtances, and that it was not much inflammable air that I was able to procure in this way. It was, however, in winter, and the heat that I could apply was not great.

SECTION X.

Of the Connexion between Alkaline Air, Volatile Alkali, and Inflammable Air.

IN ſome of my former publications I obſerved, that iron which had long ruſted in nitrous air, gave out a ſtrong ſmell of volatile alkali. I have repeatedly obſerved the ſame ſince that time, and alſo ſome other curious particulars relating to the experiment.

In all the caſes in which I had before obſerved this ſmell of volatile alkali, the iron had been expoſed to the nitrous air a very long time. But this effect is produced much ſooner by putting the iron into a weak ſolution of copper in the nitrous acid, ſuch as remains after making nitrous air, and ſuch as I now commonly uſe in order to procure dephlogiſticated nitrous air. A phial containing ſome of this iron, which had been uſed not more than once for this laſt mentioned purpoſe, having been kept cloſe corked about two months, was accidentally broken; when ſome pieces of the iron were found covered with a green ruſt, and theſe had a ſtrong ſmell of volatile alkali.

Time, however, is requiſite to produce this effect. I put ſome of this iron (which was of the turnings) moiſt, from a ſolution of copper in the nitrous acid, into a phial; and ſtopping it cloſe, left it to ſtand with its mouth immerſed in water. At that time it had no ſmell, nor had it any a month afterwards; but examining it again after two months, the ſmell of volatile alkali was pretty ſtrong.

Having, in an early period of my experiments, been much ſtruck with the change of nitrous air, in conſequence of the contact of iron, I kept ſeveral large bottles filled with ſmall pieces of iron, which I occaſionally ſupplied with nitrous air. The bottles ſhould be ſtrong ones, becauſe the iron in ruſting ſwells, and is apt to break the phials, though I do not know that this ever happened except when the iron was quite dry. The conſequence of expoſing nitrous air in this manner had always been, in the firſt place, a diminution of ſomething more than one-third of the quantity of it, in which ſtate a candle would burn in it with an enlarged flame; and when well waſhed in water it would be farther diminiſhed about one half, when the remainder would be phlogiſticated air. I had never obſerved any *increaſe* of air in theſe bottles, though ſome of them had been uſed a long time. But having neglected one of them about a year (though the mouth of it had always been kept immerſed in water) I caſually obſerved, in the courſe of laſt ſummer, that bubbles of air occaſionally iſſued from it. This ſurprized me very much, as I imagined it had not been half filled with air; and placing a veſſel to receive theſe bubbles, in about a week or ten days, I got about the quantity of an ounce meaſure. It contained no fixed air, but was all ſtrongly inflammable, and when I analyzed it, it appeared to be of that kind into which alkaline air is converted by heat.

It ſeems as if the phlogiſtcated air which is ultimately produced in this proceſs, had been decompoſed in theſe circumſtances, ſo as to form this inflammable air, but on what principle this ſhould take place, I cannot imagine.

In order to make a compariſon between the inflammable air produced from this bottle, and that which had been made from alkaline air, I exploded equal meaſures of each of them with equal quantities of the ſame dephlogiſticated air, the ſtandard of which was 0.3. When I made the exploſion with the inflammable air from alkali, the firſt diminution was to 1.2, and with that from the bottle it was 1.1. Of the former reſiduum the ſtandard was 1.1; and of the latter 0.98. Neither of them contained any portion of fixed air. On the whole, the reſemblance between theſe two kinds of air was ſo great, and the difference between them ſo ſmall, that they may be concluded to have had the ſame origin, and the ſame conſtitution.

SECTION XI.

Of the Charcoal of Metals.

MY diſcovery of the ſubſtance which I have termed the *charcoal of metals*, was, as I have obſerved, quite accidental; a copper tube having been totally diſſolved by tranſmitting through it, when it was red hot, a quantity of ſpirit of wine in vapour, all the inſide of the tube being converted into a black powdery, or friable ſubſtance. In order to make farther obſervations on the nature of this proceſs, I put the copper into an earthen tube, on which

I had not found the vapour of ſpirit of wine to have any action, though itſelf was decompoſed in paſſing through it, being chiefly converted into inflammable air.

In the firſt experiment I ſent three ounce meaſures of ſpirit of wine over two ounces of copper, in a degree of heat that kept it juſt melted. This was attended with a copious production of ſuch air as would have come from the ſpirit of wine only. But what ſurprized me moſt in the reſult was, that, though the copper had loſt no more than 28 grains of its weight, I actually collected 446 grains of the charcoal; chiefly in the form of powder, though ſome of it conſiſted of large flakes, ſeveral inches long; having been ſeparated at once from the whole ſurface of the melted copper. Theſe pieces bore handling without any danger of breaking, and were nearly quite black.

In another experiment I got 508 grains of charcoal from 19 grains of copper. But this was from copper in thin plates, and they were not all converted into perfect charcoal, there being ſomething harder, and therefore partially metallic in the centers of them.

Much of the charcoal was diſperſed and loſt in the fine black powder with which the air was loaded; and as in what I collected the copper was only about a ſixteenth part of the compoſition, I think I may venture to ſay that, in reality, it was not more than a twentieth part. In this reſpect it reſembles the charcoal of wood, or of pitcoal, in which the aſhes bear a very ſmall proportion to the inflammable air, or phlogiſton, of which the bulk of the charcoal conſiſts. This charcoal of copper was alſo as inſoluble in acids as that of wood, and it likewiſe reſembles it in other reſpects.

When the heat in which this proceſs is conducted is great, the minute diviſion, and volatility of this charcoal, is very extraordinary. Seeing it iſſue from the end of a tube in a denſe black cloud, I endeavoured to collect it in a large glaſs balloon. But after having given the balloon an uniform, but very thin black coating, not diſtinguiſhable in its appearance from *ſoot*, it iſſued from the orifice like denſe ſmoke. I then connected with it ſeveral adopters, and other veſſels, to which it gave a ſimilar coating; and laſtly, I plunged the tube out of which it finally iſſued, very deep in a veſſel of water. But ſtill the air came through the water loaded with the ſame denſe ſmoke, and very little of the matter could be collected*. I was therefore ſatiſfied, that the only way to collect any conſiderable quantity of it, was to make uſe of a degree of heat juſt ſufficient to keep the copper red hot, or rather juſt melting.

Spirit of turpentine I found to anſwer as well in the production of this charcoal as ſpirit of wine. In this proceſs I got 120 grains of charcoal from five grains of copper, notwithſtanding a very denſe black ſmoke with which the air was charged, and in which, no doubt, a great part of the charcoal was diſperſed and loſt.

---

* The incoercibility of this ſoot, in the form of vapour, very much reſembles that of the denſe vapour produced by the decompoſition of inflammable and dephlogiſticated air, of which an account was given before, both of them paſſing through water, without being retained by it.

Having procured this new ſubſtance, I proceeded to make ſeveral experiments upon it; and was much diſappointed in finding that in common air it was only melted by the burning lens, and that the heat had not (at leaſt in a ſhort time) any ſenſible effect upon it. But in dephlogiſticated air (as I ſhall deſcribe more particularly in the ſection relating to fixed air) it burned rapidly, and converted almoſt the whole of it into fixed air.

It will not be thought ſurprizing, that no ſenſible effect ſhould be produced by heating this ſubſtance in inflammable or in alkaline air. But being made red hot in the latter, the air was increaſed in bulk, and a conſiderable part of it became inflammable, as it would have been by heating any thing elſe in it.

Conſidering this ſubſtance as a *charcoal*, and being engaged at the time of my diſcovery of it, in making ſteam of water paſs over charcoal of wood, confined in red hot earthen tubes, I treated a quantity of this charcoal in the ſame manner, and the reſult was ſuch as might have been expected. A quantity of inflammable air was produced, and there remained a lighter coloured ſubſtance, which may be called the *aſhes of the metal.* Forty grains of this charcoal were reduced to eighteen by this treatment; and I collected about 200 ounce meaſures of air, which was very turbid in its firſt production, and burned with a lambent blue flame.

Having firſt produced this charcoal from *copper* (owing to my having accidentally found that the ſpirit of wine acted upon it) I proceeded to try the effect of the ſame proceſs upon other metals, beginning with *ſilver.* This metal I found to be affected very much as the copper had been; but though the matter with which the air was charged, was to appearance, equally black with that from the copper, and the veſſels in which it was received, ſo minutely divided, acquired the ſame black coating, the larger maſſes of this charcoal of ſilver were much whiter than thoſe of the copper.

Gold was not at all affected in this proceſs, not being ſenſibly changed, or diminiſhed in weight by it. At firſt, indeed, there iſſued a ſmoke of a darkiſh hue, the cauſe of which I could not diſcover, but it ſoon diſappeared.

Finding this proceſs to have ſo remarkable an effect upon copper, and none at all upon gold, I imagined that by this means, it might be poſſible to ſeparate copper from gold, ſo that we ſhould be in poſſeſſion of a new and effectual mode of *aſſaying*. But I was diſappointed in this expectation. I mixed ten grains of copper with an hundred of gold; but the copper was effectually protected by the gold from the action of the ſpirit of wine, and the whole maſs came out of the ſame weight that it had before it was ſubjected to the experiment.

I was not able to procure much charcoal from *lead*. Uſing three ounce meaſures of ſpirit of wine, and near four ounces of lead, I got only a ſmall quantity of a whitiſh powdery ſubſtance, though 58 grains of the lead were miſſing. But the inſide of the glaſs tube, through which the inflammable air was tranſmitted, was very black, ſo that a great part of the lead was probably volatilized, and diſperſed, and yet the heat that I uſed was not great.

Tranſmitting three ounce meaſures of ſpirit of wine over 360 grains of melted *tin*, it was not diminiſhed quite four grains, and the black duſt that was collected weighed 26 grains. The air was very dark.

I ſent the vapour of two ounce meaſures of ſpirit of wine over 960 grains of iron ſhavings. The reſult was, that the air was charged with black particles, and the iron was diminiſhed two grains in weight, but I was not able to collect any charcoal. The iron acquired by this means a dark blue colour.

SECTION XII.

Of Air extracted from mineral Subſtances.

HAVING formerly found that manganeſe and other natural mineral ſubſtances, yield a very pure air by extreme heat; it occurred to me that ſubterraneous fires might maintain themſelves by means of the air which they diſlodged from ſuch ſubſtances as they found in the bowels of the earth. We find that no fire can be kept burning without change of air; and that any other than common, or dephlogiſticated air, extinguiſhes fire as effectually as water. Had ſubterraneous fires been a ſimple ignition of wood, coals, or other combuſtible ſubſtances, it would have been highly probable that *air* was neceſſary for their ſupport. But we find that the greateſt heats are produced by mixtures of various ſubſtances, independently of any air that may be in contact with them. This heat excited in the bowels of the earth, from any cauſes exiſting there, may be ſufficient to volatilize any ſubſtance containing phlogiſton, and thus produce ſmoke, flame (which is inflammable air red hot) and every other phænomenon of volcanos. However, as the hypotheſis mentioned above led me to try what *kind*, and what *quantity* of air, would be yielded by various mineral ſubſtances, in great heats, it may not be uſeleſs to recite the experiments; as a knowledge of the reſults may be uſeful in other philoſophical inquiries.

As the original object of my inquiry reſpected *volcanic fires*, I gave particular attention to the examination of volcanic ſubſtances, eſpecially with a view to aſcertain whether a ſubſtance which had been in a ſtate of *fuſion* will yield air by being heated again or not; in order to diſtinguiſh the products of volcanos from other ſtony matters. Though charcoal, which has been expoſed to the moſt intenſe heat, will imbibe air from the atmoſphere, and give it out on being heated a ſecond time, yet this is not a ſubſtance that can be *fuſed*; and as this does not appear to be the property of earthly ſubſtances, ſome dependance may perhaps be placed on this teſt. If ſo, *baſaltes* can hardly be claſſed among volcanic productions, becauſe they yield more air by heat than any known *lava* that I have met with. But as the reſults of the experiments that I made both with lavas and baſaltes were various, I ſhall briefly cite them.

Of lava from Iceland 4$\frac{1}{5}$ ounces heated in an earthen retort, gave 20 ounce meaſures of air, of which one half, towards the beginning of the proceſs, was fixed air, and the remainder of the ſtandard of 1.72, extinguiſhing a candle. In the interſtices of this lava, there was a browniſh ſand, which I could not ſeparate from it.

Of lava from Veſuvius 5$\frac{1}{2}$ ounces yielded 30 ounce meaſures of air, of which the firſt portion had a ſlight appearance of fixed air, and the reſt was phlogiſticated, from the ſtandard of 1.64, to 1.38, which came laſt.

The retort was broken by the ſwelling of the maſs in cooling.

Another ounce of lava, of the conſiſtence of a hard ſtone, yielded only 3½ ounce meaſures of air, chiefly inflammable, which, I ſuppoſe, came from the gun-barrel in which this particular experiment was made.

From theſe experiments it ſeems probable, that genuine lavas do not give much air; but this will depend upon the degree of heat to which they have been ſubjected in the ſubterraneous fire.

It has been much diſputed whether baſaltes be a volcanic production, or only a cryſtalization of a maſs of matter in a fluid form. The following experiments incline me to the latter opinion.

Seven ounces of baſaltes from Scotland, heated in an earthen retort, yielded 104 ounce meaſures of air, of which the firſt portion had a ſlight appearance of fixed air, and was ſo much phlogiſticated as to extinguiſh a candle, being ſometimes of the ſtandard of 1.68.

About two ounces of the *giants cauſeway* in Ireland yielded 40 ounce meaſures of air, the firſt portion of which had a ſlight appearance of fixed air, and the reſt phlogiſticated, of the ſtandard of 1.65. It was reduced by fuſion to a hard black glaſs.

Of baſaltes from Scotland, 5 ounces 162 grains, yielded 78 ounce meaſures of air, of which no part appeared to be fixed air; but was all phlogiſticated, ſometimes of the ſtandard of 1.7, and towards the laſt 1.41.

The neighbourhood of Birmingham abounds with a ſtone which, from its being chiefly got from a village called Rowley, near Dudley, is commonly called *Rowley-rag*. When it is broken, it very much reſembles baſaltes, though it is not found in the ſame regular form. Dr. WITHERING, of this place, has given a moſt excellent analyſis of this ſubſtance, which may be ſeen in a late volume of the Philoſophical Tranſactions. All that I did with reſpect to it was, to ſubject it to a ſtrong heat in an earthen retort; and from this mode of examination it ſhould ſeem to be of the ſame nature with the baſaltes, whatever that be.

Four ounces of the Rowley-rag yielded 40 ounce meaſures of air, containing hardly any appearance of fixed air, but was phlogiſticated, of the ſtandard of 1.6, and 1.5; the laſt portion 1.31. It was reduced to a black glaſſy ſubſtance, which broke with a poliſh, exactly reſembling that which remained from the baſaltes.

The Derbyſhire *toadſtone*, in its appearance, very much reſembles the Rowley-rag, excepting that it is full of white ſpots, conſiſting of a calcarious ſubſtance. Dr. WITHERING has analyſed this, as well as the Rowley-rag, and both from his experiments and mine, they ſeem to be nearly a-kin to each other. Two ounces and 384 grains of this ſubſtance, from which the calcarious part had been diſſolved by ſpirit of nitre, yielded 60 ounce meaſures of air, the firſt portion of which contained a little fixed air, perhaps from ſome unperceived remains of the calcarious matter. The reſt was phlogiſticated, of the ſtandard of 1.7.

In another experiment, an ounce and a quarter of this ſubſtance, from which the calcarious part had been extracted firſt by oil of vitriol, and then by ſpirit of nitre, yielded 40 ounce meaſures of air, of the ſame quality of that in the former proceſs. There remained from both of them a black glaſſy matter. which ſeemed to be very liquid when it was hot, as part of it had boiled up into the neck of the retort.

Granite, like baſaltes, has been thought by ſome to be the product of volcanos, and by others to be a cryſtalization from a liquid ſtate. The latter is the opinion favoured by theſe experiments. From about an ounce and an half of this ſubſtance I got 20 ounce meaſures of air, the firſt portion of which contained a little fixed air, but the reſt was phlogiſticated, from 1.7 to 1.28, which is nearly the ſtandard of common air; but the heat was very intenſe, as the ſubſtance was reduced to a glaſs.

Again, 5 ounces and 252 grains of a blue granite yielded 70 ounce meaſures of air, of the ſame quality with the preceding. It was alſo reduced to a firm uniform glaſſy ſubſtance of a dark-brown colour. Upon the whole, therefore, it ſeems probable, that the origin of granite is ſimilar to that of baſaltes.

In Cornwall there is a ſubſtance called *elvain*, the natural hiſtory of which is very like that of granite, and the reſult of my experiments upon it ſhews that they are of the ſame nature. There is a black and white kind of elvain.

Of the black elvain one ounce and 288 grains yielded 25 ounce meaſures of air, the firſt portion of which contained a little fixed air, and the reſt was phlogiſticated, of the ſtandard of 1.54. It was melted into a browniſh black maſs.

Of the white elvain one ounce and 384 grains yielded 30 ounce meaſures of air, of the ſame quality with the preceding. It was converted into a very porous ſubſtance, exactly reſembling a pumice ſtone, but much harder.

The ſubſtance called *groan clay* is ſaid to be formed by the decompoſition of granite. Of this ſubſtance one ounce and 72 grains yielded 32 ounce meaſures of air, containing no ſenſible quantity of fixed air, but all phlogiſticated, of the ſtandard of 1.62 and 1.33. After the experiment this matter was eaſily ſhaken out of the retort, and was not ſenſibly changed in its appearance.

Such were the experiments that I made with ſubſtances that are, or are ſuppoſed to be, volcanic. Of thoſe which are certainly *not volcanic*, but which may come in the way of volcanic fires, I found thoſe into which the vitriolic acid enters to yield the greateſt quantity of pure air; but by no means ſufficient to keep alive ſuch fires as we make on the ſurface of the earth.

From ſeven ounces of *gypſum*, which I kept in a ſtrong heat twelve hours, I got 230 ounce meaſures of air, the greateſt part of which would have extinguiſhed a candle; the moſt phlogiſticated being of the ſtandard of 1.8, but it was afterwards much purer; and at the laſt conſiderably dephlogiſticated;

giſticated; for with two equal meaſures of nitrous air, the teſt was 1.3. The air was very turbid as it was produced, and the pureſt of all came rapidly, at the end of the proceſs. It is poſſible that, with a ſtronger heat, more and purer air might have been procured. The ſubſtance was reduced to *a hard maſs*, yellow next to the retort, but in the middle very white.

In this place I ſhall juſt mention the air that I got from *alum*, as a ſubſtance containing vitriolic acid, though not likely to be found in the bowels of the earth. From one ounce of *calcined alum*, very white and clean, I got 60 ounce meaſures of air, without any fixed air, or the leaſt imaginable, and ſo pure, that with two equal meaſures of nitrous air, the teſt was 1.4. Still the reſiduum had an acid taſte, ſo that with more heat, it is probable that more, and purer air, would have been procured.

The *ſtones* which I found to furniſh the greateſt quantity of air, though not the pureſt, were thoſe of the *ſchiſtus* kind, which are found in great quantities in many mountainous countries; and after being ſubjected to a very great heat, have the neareſt reſemblance to the generality of lavas of any ſubſtance on which I have yet made the experiment.

From four ounces of a *blue ſlate* I got 320 ounce meaſures of air, a very ſmall portion of which was fixed air, and the greateſt part of the reſt (the whole, I believe, except about 20 ounce meaſures) ſo impure, that the ſtandard was generally 1.8. Towards the laſt it was 1.5, and the laſt of all 1.35; ſo that a candle would juſt have burned in it. The air was very turbid, and had a very ſtrong ſmell. The ſubſtance was perfectly vitrified, and quite black, exactly reſembling lava. It then weighed, as nearly as I could gueſs (for in the fuſion it had adhered cloſely to the retort) 3 ounces and 288 grains.

From eight ounces of another kind of ſchiſtus, I got 70 ounce meaſures of air, of the ſame quality with that in the preceding experiment; and it was melted into a black maſs, harder than the former, ſo as to make a ſtill more perfect lava.

Oil of vitriol has been ſuppoſed to enter into the compoſition of *clay*. From four ounces of it I got 20 ounce meaſures of air, in every portion of which one-tenth was fixed air, and the reſt of the ſtandard of 1.72, 1.52, and at laſt 1.44.

Putting oil of vitriol to this clay, it yielded much more air, and of a better quality. Two ounces of the clay, moiſtened with this acid, gave 210 ounce meaſures of air, exceedingly turbid, containing very little fixed air, and the reſt of the ſtandard of 1.5, 1.7, 1.58, in the order in which they are here put down; but the laſt portion was 1.08, and was conſiderably dephlogiſticated.

A quantity of fine white *clay from the Apallachian mountains* gave air of the ſame kind at the beginning of the proceſs with common clay, but the retort being cracked, the experiment was interrupted.

Nothing in the form of a *ſtone* yields ſo much air as *lime ſtone*, and this is by no means all fixed air, as I believe has generally been ſuppoſed. For a very great proportion of it is more or leſs phlogiſticated, and the laſt portions often tolerably pure, ſo that a candle would nearly burn in it.

From four ounces of white *cryſtals of lime ſtone* I got 830 ounce meaſures of air, the firſt portion of which had only one-fourth of fixed air, but in the courſe of the experiment it varied, being once three-fourths, then one-half, and at the laſt one-third. The ſtandard of the reſiduum was never better than 1.56, nor worſe than 1.66.

From five ounces and a half of lime ſtone of an excellent kind, I got in all 1160 ounce meaſures of air. Of this one-tenth only was phlogiſticated, and the reſt fixed, but the laſt portion of all was half phlogiſticated.

From ſeven ounces of a tranſparent ſubſtance, found in a ſtone in the neighbourhood of Oxford, which is chiefly calcarious, I got 1280 ounce meaſures of air, of which about one-third of the whole was fixed air. The ſtandard of the reſiduum was at firſt 1.55, and afterwards 1.44.

From ſix ounces of a *blue ſtone*, found in the neighbourhood of Stratford, I got 1030 ounce meaſures of air, of which, till near the end of the proceſs, about one-half was fixed air, and at the laſt about one-fourth. The ſtandard of the remainder was about 1.6.

From three ounces of *chalk* I got 630 ounce meaſures of air, of which at the firſt one-fourth was fixed air, then almoſt two-thirds, then ſomething more than one-half, and again a little more than a third. The ſtandard of the reſiduum was from 1.66 to 1.34.

Iron ores may be pretty well diſtinguiſhed by the quality of the air that they yield by heat, as well as by their weight, and external appearance. The reſiduum of the air from other ſtony ſubſtances, after the fixed air is ſeparated from it, I have always found to be phlogiſticated, but that from iron ore is inflammable.

Three ounces and one-half of *white ſpathoſe iron ore* yielded 560 ounce meaſures of air, of which at the firſt one-third was fixed air, then only ſomething more than one-half, and again at the laſt a third. The ſtandard of the reſiduum was about 1.7, and inflammable. The ſubſtance was reduced to one

hard

hard maſs, and the bottom of the earthen retort was melted along with it.

I tried one iron ore that was of a *light* colour, and another of a *darker*. Six ounces of the lighter coloured ore yielded 750 ounce meaſures of air, of which at the firſt two thirds were fixed air, then only a little more than a half, and at the laſt a fifth. The reſiduum of the middle portions of this air only was inflammable, that of the reſt phlogiſticated; the ſtandard of it about 1.7. It was reduced to a hard black ſlag full of cavities.

Four ounces of the dark coloured ore yielded 510 ounce meaſures of air, the quality of which varied very much, like that in the preceding experiment, and the air was not more ſtrongly inflammable.

There is, I believe, ſome iron in the ſubſtance that is called *black lead* (*molybdena*) and therefore I mention the experiment that I made with it in this place. From half an ounce of it I got 25 ounce meaſures of air, of the ſtandard of 1.6, and 1.42. I have no note of any part of its being fixed, or inflammable. It had loſt only 18 grains in weight. From eight ounces and a half of another kind of black lead, I got 16 ounce meaſures of air, one-fifteenth, or one-twentieth of which was fixed air, and the reſt inflammable, burning with a blue flame.

I did not purſue theſe experiments on ores to any great extent; but having ſome *ſtream tin*, I found that 110 grains of it, gave 20 ounce meaſures of air, a ſmall portion of which was fixed air, and the remainder of the ſtandard of 1.44, and at laſt 1.34.

Of the air extracted from *mangeneſe* in glaſs retorts, I gave an account in a former volume. I now found that ſeven ounces of it, with a ſtronger heat, in an earthen retort, yielded 110 ounce meaſures of air, of which at the firſt, a ſmall portion was fixed air, and of the reſt the ſtandard was in different portions of it 1.54, 1.25, and 1.05; ſo that the bulk of it was not perhaps better than common air. The ſubſtance was concreted into a hard maſs, but was looſe from the retort.

At another time, from eight ounces of another quantity of manganeſe, I got 1200 ounce meaſures of air, of which one third was fixed air, and the reſt inflammable, burning with a blue flame. To what circumſtance this remarkable difference in the quantity and quality of the air, from what was called the ſame ſubſtance, was owing, I cannot tell. Something elſe beſides manganeſe, as I ſuſpect, muſt have been mixed with it in this experiment.

I ſhall conclude this ſection with an account of the examination of ſome other ſubſtances, of which the reſults are not always of much conſequence.

From two ounces and one-fifth of *ſteatites*, I got 43 ounce meaſures of air, which had the ſlighteſt appearance of containing fixed air. The remainder was thoroughly phlogiſticated, except that, at the laſt, it was of the ſtandard of 1.65. It came out of the retort a yellow maſs, but powdery, as it was put into it.

Two ounces of *terra ponderoſa*, gave 26 ounce meaſures of air, without any mixture of fixed air, the ſtandard of it 1.62, 1.42, and 1.29. The ſubſtance was concreted into one maſs, but was eaſily broken by ſhaking the retort, and then it did not appear to be changed in its external appearance.

Two ounces of *black woad* from Derbyſhire, yielded 80 ounce meaſures of air, no part of which was fixed air, but all better than common air, the ſtandard of it being 1.05. This circumſtance may help to account for this ſubſtance taking fire, and burning as it does, when it is mixed with linſeed oil. For if by any means it is ſo far heated, as to give out its pure air, this muſt aſſiſt the combuſtion; and the chymical attraction between the phlogiſton in the oil, and the dephlogiſticated matter in the woad may, without its aſſuming the form of air, be the cauſe of the maſs becoming hot.

Seven ounces of *fluor* gave eight ounces of air, a ſmall proportion of which was fixed air, and the reſt of the ſtandard of 1.45. It was melted into a hard maſs. Six ounces of white fluor yielded in all ten ounce meaſures of air, of which the ſlighteſt portion imaginable was fixed air, the reſt of the ſtandard of 1.34, and 1.3. In this experiment the bottom of the retort was quite diſſolved. N. B. there was no appearance of fluor acid in the water in which this air was received, and the melted maſs gave fluor acid air with oil of vitriol.

From four ounces of a kind of *ſand-ſtone*, I got 75 ounce meaſures of air, a ſmall portion of which was fixed air, the ſtandard of the reſt, for the moſt part, 1.75, and at the laſt 1.35. When taken out of the retort, it weighed three ounces and three-fourths. That part of it which was next to the bottom of the retort was whiter than the reſt, but very hard, adhering to it; and, what was pretty remarkable, the remainder had acquired juſt as firm a texture as it had before it was pounded for the purpoſe of the experiment.

Five ounces of a fine *white ſand-ſtone*, yielded about ten ounce meaſures of air, containing a little fixed air, and the reſt of the ſtandard of 1.6. This alſo was again reduced to a ſtone quite as compact as it had been before it was pounded.

Six ounces of another ſand-ſtone yielded 102 ounce meaſures of air, of which a very ſmall portion was fixed air, and the reſt of the ſtandard of 1.57, and 1.35. This alſo was reduced to a hard dark coloured ſtone, having ſeparated itſelf from the retort about a quarter of an inch, except at the bottom where it adhered to it.

From one ounce and 175 grains of *belemnite*, I got 320 ounce meaſures of air, of which at the firſt one-ſixteenth was fixed air, the reſt of the ſtandard of 1.75, 1.55. All the air came while the heat was very moderate.

From four ounces of *cryſtals of quartz*, I got 25 ounce meaſures of air, a very ſmall portion of which was fixed air, the reſt being of the ſtandard of 1.8, and 1.44.

From ſeven ounces of a *granulated quartz*, I got about ten ounce meaſures of air, containing a little fixed air, and the reſt of the ſtandard of 1.42. It came out of the retort a looſe friable ſubſtance, weighing ſix ounces 290 grains. The retort was cracked, or more air would probably have been procured.

From one ounce and 84 grains of *mica*, I got twelve ounce meaſures of air, of which no part was fixed air, but of the ſtandard of 1.4, and 1.35.

From 120 grains of *talc*, I got a quantity of air, but the retort being cracked at the beginning of the proceſs, I took no account of the quantity. Part of it was evidently fixed air, and the reſt of the ſtandard of 1.4, and a candle burned in it. The ſubſtance was reduced to a dark hard cinder, adhering to the retort.

From four ounces 355 grains of *cryſtalized glaſs*, in the form of a whitiſh ſtone, I got twelve ounce meaſures of air, which contained no fixed air, and of the ſtandard of 1.42, 1.36, and 1.31. Perhaps I uſed a greater degree of heat than the glaſs had been ſubjected to before. Otherwiſe this experiment might help to account for lava giving ſome quantity of air, though it had been in a ſtate of fuſion, having afterwards cryſtalized, like this glaſs.

The laſt experiment that I ſhall mention was made with *pit coal.* Three ounces of ſuch coal as we have at Birmingham, gave 700 ounce meaſures of air, of which I could not be ſure that any portion was fixed air. It was all inflammable, the firſt portion of it burning with a white lambent flame, and the laſt with a blue one.

SECTION XIII.

Experiments on Charcoal.

THE examination of *charcoal* is now conſidered as an object of peculiar importance in chymiſtry; and ever ſince I had diſcovered it to be one of the beſt conductors of electricity I have given particular attention to it. The moſt important of the experiments that I have made reſpecting it ſince my former publications, will be found in the ſection relating to phlogiſton, in which an account is given of the decompoſition of it by ſteam in a ſtate of ignition. In this ſection I ſhall comprize a variety of miſcellaneous obſervations, ſome of more, and others of leſs importance. Some of them alſo are repetitions of former experiments, but made with a better apparatus, and with an attention to more circumſtances.

The quantity of air to be expelled by heat from dry wood I have frequently eſtimated; but having now, by the aſſiſtance of Mr. WEDGWOOD, the advantage of doing every thing of this kind in very compact earthen retorts, which themſelves give no air, it may be worth while to mention, that from five ounces of dry oak I got 650 ounce meaſures of air, of which about one-half, nearly a third, that came at firſt was fixed air, the remainder being inflammable, and the laſt portions wholly ſo.

The property that charcoal has of abſorbing air is a remarkable circumſtance, firſt diſtinctly obſerved by the Abbe FONTANA; but ſtill there are ſeveral particulars relating to this experiment, ſuch as the *time* in which the air is imbibed, and the quality of it when it is again expelled by heat, &c. that are not undeſerving of notice. And though I have not purſued this ſubject with much regularity, I have occaſionally, and at different times, made obſervations of this kind, of which I ſhall here give an account; propoſing to reſume the experiments, and to purſue them farther.

From 789 grains of charcoal, from which all air had been expelled, and which had been expoſed to the atmoſphere, I got 30 ounce meaſures of air, no part of which was fixed air, but all phlogiſticated, extinguiſhing a candle; being of the ſtandard of 1.7. Probably pure air had been imbibed in preference to any any other; becauſe when it has been made to imbibe dephlogiſticated air only, it comes out again partially phlogiſticated. I found, however, conſiderable varieties in the quality of air emitted by charcoal, after being expoſed to the open air, as well as other circumſtances of ſome conſequence relating to the experiment.

From 680 grains of charcoal, which had been heated four times before, I got 40 ounce meaſures of air, of which the ſlighteſt portion imaginable was fixed air. Of the reſt, towards the middle of the proceſs, the ſtandard was 1.48, and the laſt, 1.52. The next day, without changing the retort, or moving it from its place, having only left it with its mouth open, to give it an opportunity of attracting more air, I heated it again, and got about two ounce meaſures of air, of the ſtandard of 1.5. This was much leſs than I had expected. I then tied a bladder to the mouth of the retort the moment it ceaſed to give air. But though the charcoal was ſhaken out of the retort into the bladder when it was cold, very little air had been abſorbed by it. It then weighed 560 grains. But having been thus expoſed to the open air, though for a ſhort ſpace of time, on being again immediately ſubjected to heat, it gave 50 ounce meaſures of air, the ſtandard of which varied in different periods of the proceſs, in the following order: 1.54, 1.58, 1.7, 1.6, and about one-twentieth of the whole was fixed air. Why this ſame charcoal ſhould give 50 ounce meaſures of air now, and only 40 before, I cannot tell. Probably a little moiſture had been attracted by it.

Being willing to aſcertain the weight that was gradually gained by charcoal in conſequence of being expoſed to the open air, on the 4th of September I left in an open diſh charcoal freſh made from dry oak, weighing - 364 grains

The next day it weighed	390
Two or three days after,	397
The 24th of Oct. following	419
The 26th of April	421

It appears, therefore, that charcoal freſh made, only abſorbs about half as much air on its firſt expoſure to the atmoſphere as it does in a courſe of time afterwards.

Judging that this charcoal would not now acquire any more weight, I ſubjected it to heat in an earthen retort; and having got from it a quantity of air that was conſiderably phlogiſticated, found that it weighed 312 grains, but the retort appeared to be cracked.

Having left the ſame charcoal expoſed to the open air a whole year, I weighed it again, and found it to be 371 grains. That this charcoal ſhould be reduced to leſs weight than it had been at the beginning of the proceſs, I could not account for at that time; but I now do it by ſuppoſing that, together with *air*, ſome *moiſture* had been imbibed; and this would help to decompoſe the charcoal, when it was ſubjected to the fire again, as is explained in the ſection relating to phlogiſton. It may, however, be determined whether the air expelled from charcoal by heat be the air which it had imbibed, or that which was formed by the decompoſition of the charcoal by means of water. For this will be inflammable air, whereas the other, as appears by theſe experiments, will be partially phlogiſticated.

Having gone through another proceſs of expoſing charcoal to air, and then expelling air from it by heat, I ſhall here note the particulars of it.

From two ounces of pounded charcoal, on the 20th of January, I expelled, by means of a ſtrong heat, 336 ounce meaſures of air, and weighing it immediately afterwards, found it to be 756 grains. On the 23d it weighed 817 grains, after being expoſed on a plate, ſo as to lie about half an inch in depth. The air expelled from it was about one-tenth fixed air. This charcoal I expoſed to the fire ſeveral times, the laſt time on the 28th of June in the year following, immediately after which it weighed 711 grains. Some of the air that I got from this charcoal was inflammable, burning with a lambent blue flame, which ſhews that moiſture had been imbibed by the charcoal.

In a former publication I obſerved, that when charcoal has imbibed air, it will give it out again, at leaſt in part, on being plunged in water, as well as by being expoſed to heat. I again plunged into water pieces of charcoal, both perfectly and imperfectly made, after having been ſome days expoſed to the open air, and found that the air they gave out in this way was in both caſes common air. It is evident, therefore, that the degree of phlogiſtication in the air, expelled by heat, is owing to the decompoſition of the charcoal.

Having repeated the Abbe FONTANA's experiments, by introducing hot charcoal through mercury, into veſſels containing different kinds of air, and being willing to recover by diſtillation the mercury that had been imbibed by the different pieces of charcoal, I at the ſame time took ſome notice of the *air* that came from them. The quantity of air was very conſiderable, but I took no exact account of it. With reſpect to its quality, it was partially phlogiſticated, the ſtandard of it being about 1.6, though the laſt that came was inflammable.

In my experiments with reſpect to phlogiſton, I made ſeveral on the decompoſition of charcoal in the ſun, the general reſults of which are there mentioned; but as the particulars were not many, I ſhall here recite them. It is only to be obſerved, that at the time that I made them I was under a miſtake with reſpect to the origin of the air I procured, imagining it to proceed wholly from the charcoal; whereas I afterwards found that a degree of moiſture, to which I had not then attended, was neceſſary to the formation of that air. Theſe experiments were all made by the heat of the ſun in vacuo.

Four grains of charcoal treated in this manner, yielded twenty ounce meaſures of air, all inflammable, except that it barely made lime water a little turbid; but without any diminution that I could meaſure. At another time four grains of charcoal gave 24 ounce meaſures of air, no part of which was fixed air.

I entirely diſperſed one grain of charcoal in vacuo, and it gave ſix ounce meaſures of air, without the ſlighteſt appearance of its containing any fixed air. This charcoal had been long expoſed to the open air, and on that account would give out more air than it otherwiſe would have done.

Four grains and an half of charcoal gave 22 ounce meaſures and an half of air; and laſtly, three grains and three quarters gave 23 ounce meaſures and an half of air, without the leaſt portion of fixed air in it.

I ſhall cloſe this account with an experiment, in which I proceeded to take the ſpecific gravity of the air which I got in theſe proceſſes, without at the time drawing a very obvious concluſion from it. From about 2½ grains of charcoal of oak, I got 15½ ounce meaſures of inflammable air, no part of which was fixed air; and weighing this air, I found that 20 ounce meaſures of it weighed four grains and a quarter leſs than the ſame bulk of common air. According to this proportion the 15 grains and an half of inflammable air ought to have weighed 5.96 grains, which is much more than the weight of the charcoal. But though I made this very obſervation at the time, I did not then infer, that *water* muſt enter into the compoſition of this air, having no ſuſpicion that the water at the bottom of the receiver, ſeveral inches below the place on which the charcoal was expoſed to the heat of the lens, could be attracted by it. Had not ſubſequent experiments ſhewn me the real nature of this inflammable air, this experiment muſt have remained inexplicable by me. At the time I imagined, I believe, that the additional weight of the air was owing to the extraneous *water* which it had imbibed in being transferred from one veſſel to another.

Having made the preceding experiments with charcoal from *wood*, I made a ſimilar one with that from *pit-coal*, and I found that a piece of it, heated in vacuo, yielded four ounce meaſures of air, after having loſt ſomething leſs than a grain in weight. The air had in it a ſmall quantity of fixed air, but the reſt was all inflammable. The charcoal had been a long time expoſed to the common air.

After the preceding experiments with charcoal, it occurred to me to vary them by making uſe of *ſoot*; but I was much ſurprized indeed, to find that it was a ſubſtance very different from charcoal, with which at firſt ſight it might ſeem right to claſs it; for it contained a portion of pure air. A quantity of it being put into an earthen retort, and expoſed to a ſtrong heat, yielded air ſo pure, that with equal quantities of nitrous air, the teſt was 0.5, a degree of purity far exceeding that of common air. There was, however, ſome inflammable air mixed with it, which made it burn with a ſlight blue flame.

Taking ſome of this ſoot from which air had been expelled by heat, I again expoſed it to the heat of a burning lens in vacuo, and from a grain and a half of it got ſix ounce meaſures of air, all inflammable, and burning with a blue flame, without any fixed air in it. It was, therefore, after the former experiment, a true *charcoal*, but not before.

SECTION XIV.

Of the conſtituent Principles of Fixed Air.

IN the paper which I preſented to the Royal Society on the ſubject of phlogiſton, I gave a general account of ſome experiments, which ſeemed to prove the compoſition of fixed air from dephlogiſticated air and phlogiſton. I ſhall now give the particulars of thoſe experiments, and alſo others which ſeem to lead to the ſame concluſion.

The experiment upon which I then laid the greateſt ſtreſs, as appearing to me the moſt concluſive, was that with *red precipitate* and *iron filings*, the former of which yields only dephlogiſticated, and the latter only inflammable air. But the firſt experiment that I made of the kind was with *charcoal* and red precipitate, the charcoal being made with ſo great a degree of heat, that no fixed air could be expelled from it, not even when it was wholly diſperſed by the heat of the ſun in vacuo. This experiment is certainly, however, not ſo concluſive as the former; becauſe, ſince dry wood and imperfectly made charcoal yield fixed air, it may be ſaid that all the *elements* of this kind of air were contained in the moſt perfect charcoal. And though this ſubſtance alone will not, even with the aſſiſtance of water, give fixed air, it might be ſaid, that this might be effected by its treatment with other ſubſtances, without their imparting any thing to it; eſpecially as the inflammable air which is procured from charcoal by means of water appears to contain fixed air, when decompoſed with dephlogiſticated air. I think, however, that I have in a former ſection, contrary to my own firſt ſuppoſition, proved that this fixed air is really a compoſition of phlogiſton contained in charcoal, and of the dephlogiſticated air with which it was inflamed, the charcoal contributing nothing to it beſide its phlogiſton. In this place I ſhall only recite the facts concerning the production of great quantities of fixed air from perfect charcoal and red precipitate.

In order to expel all fixed air, I made a quantity of perfect charcoal from dry oak; and while it was hot I pounded it, and immediately mixing four meaſures of it with one of red precipitate, and putting them into an earthen retort, I preſently got, in no greater a degree of heat than was neceſſary to revive the mercury, a large quantity of air, half of which was fixed air. Afterwards the proportion of fixed air was leſs, and towards the concluſion of the experiment there came no fixed air at all. This reſiduum was a little better at the firſt than at the laſt, when it was of the ſtandard of 1.5.

As this air contained a greater portion of phlogiſticated air than the common air of the atmoſphere, and no ſpirit of nitre, or any thing that could yield ſpirit of nitre, was concerned in the experiment, it ſhould ſeem that phlogiſticated air may be compoſed of phlogiſton and dephlogiſticated air; though this compoſition, according to the very capital diſcovery of Mr. CAVENDISH, may be reduced to ſpirit of nitre, or rather become one element in the compoſition of that acid.

In another experiment I hit upon a better proportion of the charcoal and red precipitate for making pure fixed air. For mixing one ounce of red precipitate (which all chymiſts, I believe, are agreed to be the ſame thing with *precipitate per-ſe*) and one ounce of perfect charcoal, freſh from the retort in which it was made; and putting them into a coated glaſs veſſel, I procured from the mixture, by heat, about 30 ounce meaſures of air, the whole of which was the pureſt fixed air, leaving only about one-fortieth part not abſorbed by water, and this not inflammable, but of the ſtandard of 1.7, or almoſt perfectly phlogiſticated.

This experiment made me recollect thoſe which I had formerly made with charcoal heated in nitrous acid, in which I had always procured a quantity of fixed air. I therefore repeated the experiment with ſome of the ſame charcoal which I had uſed in the preceding experiment, on the goodneſs of which I could depend; and I found that, when it was heated in the acid, in a glaſs phial with a ground ſtopper, it gave air, one-fifth of which was fixed air. At another time I got air in this proceſs, one-half of which was fixed air. To the formation of this air, I preſume, that the phlogiſton from the charcoal and the dephlogiſticated air, which is known to be produced by heating nitrous acid, muſt have contributed.

Being then apprized of the objection that might be made to the uſe of *charcoal*, as, notwithſtanding the great heat with which it was made, containing at leaſt the elements of fixed air, I made uſe of *iron,* to which no ſuch objection could be made; and mixing an ounce of the red precipitate with an ounce of iron filings, and then heating them in a coated glaſs retort, I got 20 ounce meaſures of air, of which one-ſeventh remained unabſorbed by water. The reſiduum was of the ſtandard of 1.52, but ſlightly inflammable.

Again, from half an ounce of red precipitate, and half an ounce of iron filings, I got 26 ounce meaſures of air, of which the firſt part was pretty pure fixed air; but afterwards one-tenth of it remained unabſorbed by water. Then, increaſing the proportion of iron, I mixed one ounce of red precipitate with two ounces of iron filings, and got about 40 ounce meaſures of air, of the firſt portions of which only one-twentieth was unabſorbed by water, though towards the concluſion of the proceſs this reſiduum was greater. In this proceſs I got, in the whole, 36 ounce meaſures

ſures of pure fixed air, completely abſorbed by water, beſides what was abſorbed both in the firſt reception of the air (which was in veſſels containing water) and afterwards in transferring this air into thoſe veſſels in which the quantity of it was noted, the whole of which I ſuppoſe might be about four meaſures more. Examining the firſt reſiduum of this proceſs by nitrous air, the ſtandard of it was 1.6, and afterwards 1.7.

Having heard that it was objected to this experiment, that iron contains a quantity of *plumbago*, and that the fixed air which I procured might come from that ingredient in it (though the quantity was certainly much too great to be accounted for in that way) I made uſe of other metals, to which no ſuch objection could be made, viz. *braſs* and *zinc*, and with the ſame reſult.

With two ounces of braſs duſt I mixed one ounce of red precipitate, and in a coated glaſs retort I got from it a quantity of air, two-thirds of which was fixed air. The ſtandard of the reſiduum was 0.6; ſo that there had been too great a proportion of the red precipitate. But fixed air was produced in a quantity abundantly ſufficient for my purpoſe.

In a coated glaſs retort, I put a mixture of one ounce of red precipitate and one ounce of filings of *zinc*, and got ſome air, part of which was clearly fixed air; but the retort very ſoon cracking put an end to the experiment, and I did not think it neceſſary to repeat it. I imagine, however, that it will only be at the beginning of this proceſs that much fixed air can be procured, unleſs more precaution be uſed in conducting it. For the neck of the retort breaking quite off, there iſſued from it a ſtrong flame, which evidently aroſe from the burning of the zinc in the dephlogiſticated air from the precipitate.

As turbeth mineral gives dephlogiſticated air, as well as red precipitate, I mixed this ſubſtance with iron filings, and had a ſimilar reſult, when I heated them together in in an earthen retort. One ounce of the turbeth mineral with two ounces of iron filings, yielded about ſixteen ounce meaſures of air, of which about one-third was fixed air, and the reſt of the ſtandard of 1.5.

Another experiment which ſeems to prove the formation of fixed air from phlogiſton and dephlogiſticated air, is the expulſion of it from that *black powder* which is formed by the union of lead and mercury. This powder, I have obſerved, can only be made in pure air, which is no doubt abſorbed by the metals; and this being again expelled by heat, together with the phlogiſton which had belonged to the lead, is that, I preſume, which forms the fixed air that is found in this proceſs.

When I began to make obſervations on this black powder, I mentioned my having expelled ſome fixed air from it. This was from ſuch powder as I had found ready made; and therefore, not knowing with certainty what the compoſition of it was, I diſſolved one ounce of lead in pure mercury, and then expelled it again in the form of this black powder, which, when the running mercury was pretty carefully preſſed out of it, weighed about twelve ounces.

Then expoſing it to heat, in a coated glaſs retort, I got from it about 20 ounce meaſures of air, making allowance for the quantity of fixed air, which, as I ſuppoſed, might have been abſorbed by the water, in receiving and transferring the air before any account was taken of the quantity of it. Of this air about one-thirtieth part only was not abſorbed by water. The reſiduum I did not examine. I muſt however obſerve, that in general, beſides the fixed air, I obtained a conſiderable quantity of the pureſt dephlogiſticated air, from this black powder.

In making the black powder that was uſed in the preceding experiment, I occaſionally changed the air in the phial, in which I ſhook the mercury, by blowing into it, ſometimes with a pair of bellows, and ſometimes with my mouth; and as it was ſuggeſted that this might have ſupplied the fixed air which I afterwards found in the black powder, I diſſolved two ounces of lead in mercury, and got the black powder without blowing into the veſſel at all, only changing the air ſo much oftner as was then neceſſary. From ſix ounces of the black powder thus carefully prepared, I expelled 4½ ounce meaſures of air, of which one and a half was pure fixed air. This was ſufficient to ſatisfy me that *ſome* fixed air is certainly procured in this proceſs. The reſiduum of this fixed air was of the ſtandard of of 1.7, or 1.8. I did not at this time get from this powder all the air it would have yielded.

Being now ſatisfied that there was no occaſion to prepare this black powder with the precaution mentioned above, I repeated the experiment with ten ounces of it prepared in the readier method which I had uſed before, with a view to examine the reſiduum of the air, when the fixed air ſhould be ſeparated from it. The produce of air was in all about 23 ounce meaſures, which I received in four portions of five ounce meaſures each, and another containing the remainder. All theſe portions I examined ſeparately, obſerving the proportion of reſiduum in each of them, and the quality, as meaſured by my uſual ſtandard, and the reſult was as follows: Of the firſt portion there remained one-fourth, of the ſtandard of 1.6; of the ſecond one third, of the ſtandard of 1.44; of the third one-half, of the ſtandard of 0.8; and of the fourth three-fourths, of the ſame quality with the preceding.

In the laſt portion the reſiduum was one-half of the whole, and that I found to be ſo pure, that, mixing it with two equal quantities of nitrous air, the ſtandard of it was 0.63; ſo that the quality of theſe reſiduums was continually purer, till at the laſt it was pretty highly dephlogiſticated.

It may be inferred from both theſe courſes of experiments, that fixed air conſiſts not of inflammable air (which I ſuppoſe neceſſarily contains water) but of pure phlogiſton, and dephlogiſticated air. In the experiments with the red precipitate and iron, no water at all is concerned, unleſs either the iron itſelf contain ſome, or the mercury, or dephlogiſticated air: ſince when the red precipitate is decompoſed by itſelf, nothing is produced beſides mercury and dephlogiſticated air, without any water. The experiment with the black powder will equally authorize the ſame concluſion, as neither the lead, the mercury, nor the pure air that combines with them, can be ſaid to contain any water.

It may be worth while to obſerve that, in making this black powder, the phial in which the lead and mercury are ſhaken, grows very warm, as the coagulum of the two metals (from which the black powder is preſſed) begins to form, and that in ſqueezing this coagulum in a cloth (which is the readieſt method of ſeparating the running mercury from the black powder) it ſuddenly becomes ſo hot, that I could ſometimes hardly bear to handle it.

For the information of thoſe who may wiſh to repeat theſe experiments, I would obſerve, that in uſing as much mercury as I can conveniently ſhake, in a phial containing about three pints of water, I have got, with four hundred concuſſions of the amalgum (blowing into the phial with a pair of bellows after every hundred concuſſions) near eight ounces of the black powder.

While I am upon the ſubject of this black powder, I ſhall obſerve, that it occurred to me to mix with it more matter containing phlogiſton, in order to ſee what change that would make in the reſiduum of the produce of air.

From four ounces of the black powder mixed with two ounces of iron filings, heated in an earthen retort, I expelled 54 ounce meaſures of air, of which not more than four ounce meaſures were fixed air, and the reſiduum, examined at different times, was of the ſtandards of 1.3, and 1.44; but the greater part of it was of 1.52, ſo that there was a conſiderable production of inflammable air from the iron. In this experiment, I raiſed the heat very gradually, till I had got one third of the produce of air. This I did from an idea that this moderate heat might increaſe the quantity of the fixed air, but it did not appear to make any difference in this reſpect.

Then varying the proportion of the ingredients, I mixed 20 ounces of the black powder with only one ounce of iron filings, and receiving the air in three portions, obſerved as follows. The firſt portion, which contained ſix ounce meaſures, had a reſiduum of 3.5, of the ſtandard of 1.6. The ſecond, which was one ounce meaſure, had a reſiduum of 0.12, of the ſtandard of 1.7; and the third portion, which was only one ounce meaſure, had a reſiduum of 0.12, of the ſtandard of 1.7. Whether this was the whole of the produce of air, I do not recollect.

In order to try more fully the effect of different degrees of heat, I repeated the proceſs with the black powder, only determining to ſuſpend the proceſs in the middle of the produce of air. Accordingly I heated two ounces of the black powder in a porcelain veſſel; when I obſerved that ſome portions of the produce contained about one half fixed air, and that this proportion kept growing leſs and leſs, till the produce conſiſted of nothing but the pureſt dephlogiſticated air, the ſtandard of it being, with two equal meaſures of nitrous air, 0.2. I then let the veſſel cool, and obſerved that, on reſuming the experiment, the air came with the ſame purity to the laſt.

Examining the reſiduum in the retort, I found half an ounce of red powder, the colour of which could hardly be diſtinguiſhed from that of precipitate *per ſe.* So that, no doubt, the mercury had been converted into it, and this very pure air was probably that which came from the precipitate as it was reviving. In this way, therefore, it would be eaſy to make this precipitate in large quantities, could a method be found of ſeparating it from the *red lead,* with which it is, in this proceſs, neceſſarily mixed.

In the preceding experiment it will have been obſerved, that, at firſt, the reſiduum was conſiderably phlogiſticated, but at the laſt remarkably pure. An accident in a ſubſequent experiment I once thought had diſcovered the cauſe of this difference. In the middle of one of the proceſſes, in which I was uſing the black powder only, heating it in a glaſs veſſel, a quantity of water was drawn up through the tube that communicated with the recipient, and got into the veſſel that contained the black powder; and in all the remainder of that proceſs, the reſiduum of the air was no better than about the ſtandard of 1.7. Water came over along with this air to the very laſt, though the bottom of the veſſel was red hot. When the proceſs was over, the matter taken out of the veſſel was ſtill moiſt, and of a dark grey colour.

On this I made a paſte of the powder with water, and drying it a little, immediately repeated the experiment with it; but I found no ſenſible difference between the ſubſtance in this ſtate, and that which had not been wetted. Four ounces of it yielded 120 ounce meaſures of air, of which about 12 were pure fixed air, completely abſorbed by water, and the reſt highly dephlogiſticated. However, in one proceſs of this kind, from two ounces and a half of this powder, which had been moiſtened and dried again, I got 70 ounce meaſures of air, of which only a very ſmall part was fixed air, and the reſiduum was by no means pure dephlogiſticated air. For with two equal meaſures of nitreous air, the ſtandard was 1.2 and 1.3. At other times alſo I have had much leſs fixed air from this black powder when it had not been wetted, than in ſeveral of the inſtances above mentioned; and I have not as yet been able to diſcover the circumſtance on which the production of it in a greater or leſs quantity depends.

In the preceding proceſſes with this black powder, I always got from it more or leſs of fixed air. But thinking to produce more of it by heating this ſubſtance with a burning lens in dephlogiſticated air, I was ſurpriſed to find, that I only increaſed the quantity of dephlogiſticated air in the veſſel, and produced no fixed air at all. Whence this remarkable difference could ariſe, I do not pretend to ſay. It will be ſeen, that, in this proceſs with inflammable air, I found it to be a matter of indifference whether I uſed this black powder or the red precipitate; both of them equally imbibing inflammable air, without producing either water or fixed air.

Another deciſive proof of the generation of fixed air from phlogiſton and dephlogiſticated air, is the conſtant production of it when iron is melted in dephlogiſticated air over mercury, by means of a burning lens. This experiment being a very pleaſing one, I repeated it very often; and as it is on too ſmall a ſcale to admit of great exactneſs, I ſhall mention the reſults of ſeveral of them, obſerving, in the firſt place, that no water is produced in this proceſs.

At one time I obſerved, that when two-thirds of the deplogiſticated air diſappeared, one-ninth of the remainder was fixed air. At another time ſeven ounce meaſures of dephlogiſticated air were reduced to two, of which more than one-third was fixed air. Again, ſix ounce meaſures and a half were reduced to half an ounce meaſure, and one half of this was fixed air. I repeated the experiment again, at the ſame time, and had the very ſame reſult.

In all theſe experiments it cannot be doubted but that the greater part of the dephlogiſticated air incorporates with the iron, converting it into a *ſcale*, or *finery cinder*, being the very ſame ſubſtance with that which is produced by tranſmitting ſteam over iron when it is red hot; but at the ſame time ſome phlogiſton muſt be expelled from the iron, and unite with the dephlogiſticated air in the veſſel, in order to form the fixed air that is found in it; as in other caſes it unites with *water*, and makes inflammable air.

Perhaps as deciſive a proof as any of the real production of fixed air from phlogiſton and dephlogiſticated air, may be drawn from the experiments in which I always found a quantity of it when I burned ſulphur in dephlogiſticated air. In one of thoſe experiments to which I gave more particular attention, ſix ounce meaſures and an half of the dephlogiſticated air were reduced to about two ounce meaſures, and one-fifth of this was fixed air. Much vitriolic acid air had been produced in this proceſs: For, before I admitted any water to it, the ſix and an half ounce meaſures were only reduced to ſix. When both the vitriolic acid and the fixed air were abſorbed by water, the remainder was very pure dephlogiſticated air, the ſtandard of it being 0.3.

I had always concluded, that no fixed air could be produced by the decompoſition of inflammable air, which had been procured by means of the mineral acids, becauſe I had not been able to do it with that which I had got by means of the vitriolic acid; but I learned from Mr. METHERIE (in whoſe ingenious treatiſe on air I cannot help wiſhing he had indulged a little leſs to theory) that this is peculiar to the vitriolic acid, the remains of which, diffuſed through the inflammable air procured by it, he conjectures, may decompoſe the fixed air actually produced in the proceſs. See his Treatiſe, p. 110. For, as I have hinted before, when the inflammable air is produced from iron, by means of ſpirit of ſalt, there is a very perceivable quantity of fixed air, when it is united with dephlogiſticated air. When I decompoſed theſe two kinds of air in equal quantities, they were reduced to about 0.5 of a meaſure, and of this not more than about one fortieth part was fixed air. This experiment, ought, however, to be added to the other proofs of fixed air being produced by the union of dephlogiſticated air and phlogiſton.

The laſt inſtance of the generation of fixed air from phlogiſton and dephlogiſticated air, which I ſhall mention in this ſection, is of a much more ſtriking nature than any that I have yet recited. Having made what I call *charcoal of copper*, by making vapour of ſpirit of wine paſs over copper when it was red hot, I took a piece of it, and, with no very particular view, heated it in different kinds of air. Among others, I did this in common air, and not obſerving any increaſe or decreaſe of the quantity of air, concluded, but perhaps too haſtily, that no change was made in it. For when I repeated the experiment in dephlogiſticated air, the charcoal burned very intenſely; and when a part of it was conſumed (which, like common charcoal in the ſame proceſs, was done without leaving any ſenſible reſiduum) I found that no heat which I could apply afterwards had any farther effect on what was left of the charcoal. Concluding, therefore, that ſome change muſt be made in the quality of the air, I examined it, and found about nine-tenths of it to be the pureſt fixed air, and the reſiduum was ſuch as would have been made by ſeparating the abſolutely pure part of the dephlogiſticated air, and leaving all the impurities in what remained.

Having aſcertained this fact, I repeated the experiment, weighing the piece of charcoal very carefully before and after the proceſs, and then found that, by the loſs of one grain of the charcoal, I reduced four ounce meaſures of dephlogiſticated air till one-ninth only remained unabſorbed by water; and again, with the loſs of one grain and an half of the charcoal, I reduced ſix and an half meaſures of dephlogiſticated air till five and a half ounce meaſures were pure fixed air.

In this proceſs there was a diminution of the bulk of the air after the experiment, as might be expected from the change of the air into one of a heavier kind by means of a ſubſtance, or principle, that could not add much to the weight of it; but I did not accurately meaſure this. In one of the experiments 4.3 ounce meaſure of dephlogiſticated air were diminiſhed, I obſerved, about one-thirtieth part of the whole. But being in a pretty wide veſſel, ſuch a meaſure cannot be accurate enough for computation. In this caſe, when the fixed air was ſeparated by water, there was a reſiduum of 0.75 of a meaſure of the ſtandard of 1.0, whereas the dephlogiſticated air before the experiment had been of the ſtandard of 0.2.

That dephlogiſticated air actually enters into the compoſition of this fixed air in this experiment, is evident from the weight of the fixed air, which far exceeds that of the charcoal, which is diſperſed in the proceſs. For in this laſt experiment the weight of the fixed air produced was 4.95 grains. Conſequently, ſuppoſing the charcoal to be wholly phlogiſton, as it is very nearly ſo, fixed air may be ſaid to conſiſt of 3.45 parts of dephlogiſticated air, and 1.5 phlogiſton. So that the dephlogiſticated air is more than three times the proportion of the phlogiſton in it.

I muſt not conclude this ſection without obſerving that, in one experiment, I never failed to produce fixed air, though it is not eaſy to ſee how *one* of its ſuppoſed elements, viz. dephlogiſticated air, could enter into it. This is by heating iron in vitriolic acid air. I repeated the experiment many times, and always had this very remarkable reſult.

In one of the experiments, four ounce meaſures of the vitriolic acid air were reduced to 0.65 of an ounce meaſure; and of this three and one-half of the whole was fixed air, abſorbed by lime-water, and the remainder was ſlightly inflammable. In another experiment I could not perceive any thing inflammable in the reſiduum. It appeared to be only phlogiſticated air. But theſe reſiduums are always ſmall, ſo that it is not eaſy to diſtinguiſh weakly inflammable air from that which is phlogiſticated.

SECTION XV.

Of the Change made in Fixed Air by the electric Spark.

I Have obſerved in my former publications, that by taking the electric ſpark in fixed air, a part of it is converted into air that is not abſorbed by water. I have ſince repeated this experiment with more care; and though I have never been able to make the whole of any proportion of fixed air immiſcible with water by this means, yet I have always ſo far changed it, that the reſiduum was more conſiderable than before, but in different proportions.

I took the electric ſpark about two hours in a ſmall quantity of fixed air confined in a glaſs tube by mercury. Before the experiment, one-thirtieth of the air was unabſorbed by water, but afterwards one-fourth. The glaſs tube in which this experiment was made became very black in the inſide; and as this change is made in mercury by the addition of phlogiſton, it looks as if ſome of the phlogiſton, which had made a part of the fixed air, had, by this proceſs, been ſeparated from it; and leaving a greater proportion of dephlogiſticated air in the remainder, would neceſſarily make it leſs miſcible with water. The blackneſs on the inſide of the tubes, in which the electric ſpark is taken through vitriolic acid air or common air, I before diſcovered to be mercury ſuperſaturated with phlogiſton.

The next time that I repeated this experiment, I attended to the quality of the reſiduum before and after the proceſs; and the reſult was ſuch as ſeems to confirm the above-mentioned conjecture. I took the electric ſpark an hour and ten minutes in little more than half an ounce meaſure of fixed air, after which one-fifth of the whole was unabſorbed by water, and the ſtandard of the reſiduum was 0.9. Of the original fixed air about one-thirtieth was unabſorbed by water, and the ſtandard of the reſiduum was 1.0. In this experiment I alſo obſerved that the quantity of the air in which I made the experiment was increaſed about a twentieth part, which I do not pretend to explain.

Again, I took the electric ſpark an hour in half an ounce meaſure of fixed air, after which there remained as much reſiduum unabſorbed by water as had remained in about five times the quantity of the ſame fixed air in which no ſpark had been taken. This reſiduum was alſo much purer than that of the original fixed air, the ſtandard of it being 0.8, whereas that of the original fixed air had been, as before, 1.0. I repeated the experiment, and found the reſiduum ſtill greater, but of the ſame pure quality; and in this caſe I obſerved a good deal of the black matter adhering to the inſide of the tube.

In the following experiment I obſerved a farther change in this ſubſtance. In a ſmall tube, containing about one-fifteenth of an ounce meaſure of fixed air, I took the electric ſpark about an hour; after which there was a good deal of the black matter clouding all the inſide of the tube; but the lower part of it was covered with ſomething of a yellow colour, like ſulphur. In this caſe the reſiduum not abſorbed by water was between one-fourth and one-fifth of the whole, and leſs pure than the former reſiduums. Had not the dephlogiſticated air in the fixed air paſſed into the mercury, tending to make it a precipitate *per ſe*? Was not this the cauſe of the reſiduum being leſs pure than before? And does not this experiment alſo prove, that phlogiſticated air may be compoſed of the ſame materials with fixed air, viz. dephlogiſticated air and phlogiſton?

Again, I took the electric ſpark three hours in a ſmall quantity of fixed air, and obſerved that it was firſt increaſed, and then diminiſhed about one-eighth of the whole; the inſide of the tube being very black, and below the mercury very yellow, about the ſpace of a quarter of an inch quite round the tube. But that ſpace, or at leaſt part of it, had been above the mercury at the beginning of the proceſs. There remained one-third of the air unabſorbed by water, and ſo impure, that the ſtandard of it was 1.8.

To vary the experiment, I took the electric ſpark in a quantity of fixed air confined by water, impregnated with fixed air. The quantity was much increaſed by the air extricated from the water, and after the proceſs by far the greater part of it was incapable of being abſorbed by lime water. In the courſe of this experiment, I obſerved that water impregnated with fixed air is by no means ſo good a conductor of electricity as water impregnated with any of the mineral acids.

Again I took the electric ſpark in fixed air, confined by a little common water, and obſerved that the blackneſs mentioned above extended more than a quarter of an inch below the ſurface of the mercury, in the ſame manner as the yellow colour had done before. In this caſe alſo, the reſiduum was purer than that of the original fixed air.

Again I took the electric ſpark half an hour in ſeven tenths of an ounce meaſure of fixed air, after which one-tenth of it was immiſcible with water, and the reſiduum was evidently better than the natural reſiduum of the ſame fixed air. The ſtandard of that had been 1.0, and of the other about 0.85.

I took the electric ſpark three hours in about three fourths of an ounce meaſure of fixed air, after which it was increaſed in bulk one eighteenth. Water being admitted to it, there remained one-ſixth unabſorbed. Being examined, the ſtandard was found to be as before, a little better than the reſiduum of the ſame fixed air.

Being deſirous of aſcertaining whether this change in the conſtitution of the fixed air was owing to the *light*, or the *heat* produced by the electric ſpark, or to ſomething peculiar to electricity. I firſt threw a ſtrong light by means of a burning lens, on ſome pounded glaſs, confined in fixed air, for ſome hours. But though the reſiduum was by this means a little increaſed, yet being of the ſame quality with the common air, I ſuſpected that it was the air which was neceſſarily introduced through the quickſilver along with the pounded glaſs. There was no change in the dimenſions of the air after the experiment.

I repeated the proceſs with fine glaſs-houſe ſand, which had been previouſly expoſed to a ſtrong heat. But though the reſiduum was increaſed, the experiment was not, upon the whole, more ſatisfactory than the former. I alſo heated bits of crucibles in the ſame manner, and found the reſiduum larger than before, in the proportion of 10 to 6.6; but the quality of it was worſe.

To what this ſhould be owing, I cannot tell.

I once more repeated the experiment with bits of crucibles, and the reſult was certainly favourable to the hypotheſis of a real change being made in the quality of the air by *heat*, but I do not pretend to ſay that it was deciſively ſo. After the proceſs with fifty-ſix meaſures of the air there was a reſiduum of three meaſures; whereas before the experiment, the ſame quantity of the fixed air had left a reſiduum of only two meaſures. And that the additional meaſure was not the common air, introduced into the veſſel by adhering to the bits of crucibles, was evident from the quality of the reſiduum, which was the very ſame, viz. of the ſtandard of 1.1. I alſo aſſured myſelf that there was no fallacy of this kind in the experiment, by introducing the very ſame bits of crucibles into another equal quantity of fixed air. For I did not find that any ſenſible quantity of common air had been carried into the veſſel along with them.

However, by heating *iron* in fixed air, there can be no doubt but that a ſenſible quantity of it is converted into phlogiſticated air; which agrees with the experiments that I formerly made, by putting pots of iron filings and brimſtone into fixed air. The experiments that I made of this kind were the following, in which it will be obſerved, that, though in ſome of them, there was an increaſe of the quantity of air after the proceſs, yet that it was by no means equal to the quantity that remained unabſorbed by the water; and therefore, there muſt have been a farther addition made of this kind of air in the proceſs.

After heating turnings of malleable iron in a quantity of fixed air for ſome time, I examined a part of it, and found that about one-tenth of the whole was immiſcible with water. Having reſumed the proceſs with the remainder, I found a reſiduum of one-fourth of the whole. There ſeemed to be a ſmall addition to the quantity of air after the firſt part of the proceſs, but I could not perceive that there was any after the ſecond. I reſumed the proceſs a third time, but did not find that I had made more than one fourth of the whole immiſcible with water. At another time I heated the ſame kind of iron in fixed air, till of $3\frac{3}{4}$ ounce meaſures of air there was a reſiduum of 0.8 of a meaſure, which was ſlightly inflammable, burning with a blue flame; and in this caſe there was no ſenſible addition to the quantity of air at all. Laſtly, I heated iron in three ounce meaſures of fixed air till there was an addition of 0.4 of a meaſure to the quantity of it; but there was a reſiduum of one and a half of a meaſure not abſorbed by water, which burned with a ſlightly exploſive blue flame.

SECTION

SECTION XVI.

Of the Quantity of Fixed Air expelled from various Subſtances.

AS it is ſometimes uſeful to know what quantity of fixed air may be expelled from certain ſubſtances in particular ſtates, and alſo what proportion of other kinds of air may be mixed with it, I ſhall mention ſuch proceſſes of this kind as I have notes of. They are ſimilar to ſome of thoſe in which air was extracted from mineral ſubſtances.

The pureſt calcareous earth is *chalk*, and the moſt perfect chalk is that which is called *whiting*, which is therefore uſeful in many experiments, ſo that it is worth while to know what air it contains. From ſeven ounces of this ſubſtance, I got, in an earthen retort, 630 ounce meaſures of air, by which it was reduced to four ounces.

Every portion of the air contained about one-third that was not fixed air, the ſtandard of which was 1.36, 1.38. Again, from ſix ounces of whiting, I got 440 ounce meaſures of air, about half of which was fixed air, and the remainder of the ſtandard of 1.4. The whiting was reduced to three ounces and 312 grains.

In order to try whether any peculiar kind of air might be procured from whiting ſaturated with acids, I moiſtened ſome, which had been well calcined, with water impregnated with vitriolic acid air; and then by heat expelled from it 90 ounce meaſures of air, the former part of which was more than three-fourths fixed air, and the reſiduum of the ſtandard of 1.5. The laſt portion had leſs fixed air in it, and the ſtandard of the reſiduum was 1.44. The ſubſtance was rendered black and hard, but in ſpirit of ſalt it became white and ſoft.

When *quick lime* is ſuffered to fall in the open air, it firſt attracts moiſture, and then that moiſture gives place to fixed air. From three ounces and a quarter of this fallen lime I got 375 ounce meaſures of air, of which about one-fifth was fixed air, and the ſtandard of the reſiduum was 1.4.

From four ounces of *white lead* I expelled 240 ounce meaſures of air, before the retort was diſſolved by it. Of the firſt produce there remained one-third, not fixed air, of the ſtandard of 1.36; and towards the laſt, the reſiduum was of the ſtandard of 1.28, when with the common air it was 1.23.

From two ounces and three quarters of *wood aſhes* I got, in a very ſtrong heat, 430 ounce meaſures of air, of the firſt portion of which one-tenth, of the ſecond one-third, and of the third one-half was fixed air. The reſiduum of the ſecond portion was of the ſtandard of 1.6, and that of the third 1.7. It extinguiſhed a candle; ſo that the air came properly from the *aſhes*, and not from any remaining particles of the charcoal mixed with them. After the proceſs, the aſhes weighed 839 grains. Being expoſed to the open air one day, they weighed 842 grains, and, perhaps with more heat than before, yielded 50 ounce meaſures of air, of which about an eight was fixed air, and the ſtandard of the reſiduum was 1.38, and 1.41. A candle burned in it; ſo that it is evident ſome of the dephlogiſticated part of the atmoſphere had been imbibed by theſe aſhes. They then weighed 789 grains and a half.

From three ounces of *pit-coal aſhes*, I got air, the ſtandard of which was 1.7, and extinguiſhed a candle. I took no note of the quantity of fixed air, and through an accident in the proceſs moſt of the air eſcaped.

HOMBERG's *pyrophorus* is a ſubſtance liable to be ſpoiled by expoſure to the air. I prepared ſome of it, with a view to obſerve what it imbibed from the air, and with it I made the following experiments. It was compoſed of two parts burned alum, one of ſalt of tartar, and one of charcoal. About two ounce meaſures of this pyrophorus I ſuffered to burn in the open air; and then in an earthen retort, I extracted from it 144 ounce meaſures of air, of which one half at the firſt was fixed air, but at the laſt very little. The reſiduum of the firſt portion extinguiſhed a candle, but that of the laſt burned with a lambent blue flame. When examined with nitrous air, both the reſiduums were of the ſtandard of about 1.8.

The pyrophorus was then kept two days in the retort, with its mouth in mercury, and then being taken out, it preſently grew hot, and burned as well as ever. Immediately before the burning it weighed 428 grains, immediately after it 449 grains. Having been ſpread thin, and expoſed to the atmoſphere, the next morning it weighed 828 grains; but when well dried, it weighed only 486 grains. It was then ſubjected to a greater heat than before, and it yielded 110 ounce meaſures of air, the firſt portions of which were half fixed air; but the laſt contained very little, and burned with a lambent blue flame. The ſubſtance then weighed 396 grains.

Afterwards I took a quantity of pyrophorus, which would not take fire in the open air, and heating it in an earthen retort, found five-ſevenths of the firſt part of the produce fixed air; but this proportion gradually diminiſhed, till at laſt nine tenths of the whole was inflammable air, burning with a lambent blue flame. This inflammable air being decompoſed with an equal quantity of dephlogiſticated air, yielded 0.86 of a meaſure of fixed air.

Another quantity of pyrophorus, which had burned very well, and which, during the burning, and in two days expoſure to the atmoſphere afterwards, had acquired 132 grains in weight, being again expoſed to heat, in an earthen retort, gave an hundred and eighty ounce meaſures of air, of which the firſt portion was three-ſevenths of fixed air, and the reſt phlogiſticated. But afterwards one-half only was fixed air, and the reſt inflammable, burning with a lambent flame, and at laſt it was wholly inflammable. When this pyrophorus was cool, it took fire again by expoſure to the open air, but not without the aſſiſtance of ſome external heat. It had been red hot through its whole maſs at the firſt burning of it, and had continued ſo a conſiderable time, after which the ſurface of it was ſlightly covered with white aſhes; but all the inſide was as black as ever it had been.

From four ounces of dry *ox blood* I got 1200 ounce meaſures of air, and I conjectured that not leſs than 200 ounce meaſures eſcaped. It contained no fixed air. The firſt portion of it burned with a large lambent white flame, the middle portion fainter, and the laſt was hardly inflammable at all, but had a ſlight blue flame. What remained of the blood weighed 255 grains, and was a very good conductor of electricity, which is not uſually the caſe with the charcoal of animal ſubſtances.

SECTION XVII.

Experiments relating to Dephlogiſticated Air.

SINCE the diſcovery of dephlogiſticated air, I have been led to form different opinions concerning the conſtitution of it. At firſt I thought it conſiſted of earth and ſpirit of nitre, becauſe, in every proceſs from which I then procured it, I uſed ſome kind of earth, but eſpecially the metallic ones, moiſtened with ſpirit of nitre, and there was a loſs of the earth, as well as of the acid, in every proceſs. Afterwards I was convinced, that the earth was only diſperſed in the proceſs, and therefore concluded, that this air was merely *nitrous* acid in another form, with the addition of the principle of *heat*, or whatever elſe might be neceſſary to give it the form of air. Since that time Mr. WATT has led me to think, that there is no acid whatever in dephlogiſticated air, and that it conſiſts of nothing more than one of the conſtituent parts water, combined with the element of *heat*; the other conſtituent part of water being inflammable air. In this opinion I for the preſent acquieſce; and in this ſection I ſhall recite ſome experiments which favour this hypotheſis, as well as others of a miſcellaneous nature, relating to this ſpecies of air.

The firſt obſervation that I ſhall make reſpects the *production* of this air. At firſt I procured it from precipitate *per ſe*, which is an expenſive article in chymiſtry, and then from red precipitate, which, when carefully prepared, ſo as to expel from it all the acid, is the very ſame thing. I have frequently obſerved that an ounce of this ſubſtance will yield 60 ounce meaſures of the pureſt dephlogiſticated air; and it is ſomething remarkable, that an ounce of *turbith mineral*, which conſiſts of mercury and the vitriolic acid, yields about the ſame quantity of air, and equally pure.

For a conſiderable time I was not able to procure dephlogiſticated air by means of any other acid than the nitrous. But afterwards I got it by means of the vitriolic, as in the caſe of turbith mineral. Other perſons have procured it by means of the marine acid, as Mr. LANDRIANI, who firſt ſucceeded in this proceſs, and then Mr. BERTHOLET. I have now procured this air by means of the *acetous acid*, and though the fact is but of little conſequence, and the experiment was made with another view, it may not be amiſs to recite the particulars of it.

Having mixed half an ounce of *radical vinegar* with two ounces of calcined whiting, I got from it, by heat, in an earthen retort, 350 ounce meaſures of air, of which about one-third was fixed air, but more than this in the firſt portions, and leſs in the laſt. The ſtandard of the reſiduum in the firſt portions was 1.66, in the ſecond 1.42, and in the third 1.38; which was very near the purity of common air. The whiting then weighed 760 grains. I then put a quarter of an ounce more radical vinegar to it, and without taking any account of the quality of the air, only obſerved that I got 120 ounce meaſures from it, and that the reſiduum weighed 730 grains. After this I put to it a quarter of an ounce more of the ſame acid, and got from it 140 ounce meaſures of air, of which the laſt portion had in it no fixed air, and was conſiderably better than common air; but of the exact ſtandard of it I find no note. The whiting then weighed 489 grains. The air had always been very turbid, which accounts for the continual loſs of weight in theſe proceſſes.

The cheapeſt method of procuring dephlogiſticated air in large quantities that we are yet acquainted with, is to extract it from nitre by means of heat; and in general no more has been got from an ounce of nitre than about 100 ounce meaſures of air. The reaſon of which has been, that, in order to get it pure, *glaſs veſſels* have been uſed; and after a part of the acid has been expelled, the remaining alkali has diſſolved the glaſs, and the proceſs has of courſe terminated. But by means of Mr. Wedgwood's excellent earthen retorts, I have been able to get ſuch quantities of this air from nitre, as to myſelf, and all my friends, appeared very extraordinary; and has aſſiſted Mr. Watt in proving that there is no acid in this ſpecies of air. As only the general reſults of theſe experiments have been quoted by Mr. Watt, and others, I ſhall recite ſome of the particulars in this place.

The firſt time that I got more air than uſual from nitre was in a coated glaſs retort, when, from two ounces of it, I got 300 ounce meaſures of air. But from the ſame quantity of *purified nitre*, in one of Mr. Wedgwood's retorts, I got 787 ounce meaſures. The air procured in this manner is never of the pureſt kind; and the ſtandard of this, from the firſt to the laſt, with two equal meaſures of nitrous air, was only 1.25. I conducted this proceſs ſo that the air was very little turbid, and yet there was ſomething acid diſcovered by the ſmell in every portion of it. In another proceſs I got 796 ounce meaſures of air, and the retort was diſſolved before the proceſs was completed. But the largeſt quantity that I ever got from two ounces of nitre was 812 ounce meaſures, and this was purer than any I had got before in this way, the ſtandard of the greateſt part of it being 0.95, though at the very laſt 1.5. In all caſes I have obſerved, that the purer the air is, the greater is the quantity. For it can only be rendered impure by ſome phlogiſtic matter uniting with the pureſt part of it, and forming, in many caſes, as I am inclined to think, phlogiſticated air. In this experiment the loſs in the nitre was 531 grains. The air would have weighed ſomething more than 487 grains, ſo that more than one-half of the weight of the nitre was reduced to the form of air; and there remained 34 grains for the water of cryſtallization, &c.

In theſe experiments it was a matter of ſome conſequence to determine whether any *acid* remained in the nitre after all the air was come over; and as the vitriolic acid has a ſtronger affinity with the baſe of the nitre, there appeared to be no better method of determining this, than to put oil of vitriol to it, and endeavour to make nitrous acid. Accordingly, in the former proceſſes, I had always been able to get nitrous acid, or nitrous air, from what remained of the nitre, after the dephlogiſticated air was expelled from it. Thus from what remained of one of the preceding proceſſes, I had got nine ounce meaſures of nitrous air from half an ounce of the ſaline reſiduum. And as it was evident that the nitrous acid in this nitrous air was much leſs than had been contained in the nitre, I had been uſed to conclude, that the acid had been converted into dephlogiſticated air. But I was led to ſuſpect this concluſion, from obſerving that the dephlogiſticated air had nothing ſenſibly acid in it, not only while it exiſted in the form of air, but alſo when decompoſed with inflammable air; and I had always obſerved, that every veſſel of the air, freſh received from the nitre, had a pungent ſmell, which muſt have proceeded from ſome volatile acid diffuſed through it; and alſo the water in which the air was received had acquired a ſlight acidity, though it was of a very volatile nature.

To theſe circumſtances I gave particular attention in my future experiments. To that which remained of the laſt mentioned experiment, in which I procured 812 meaſures of air from two ounces of nitre, I put oil of vitriol and water. But though it was expoſed to heat till the water was diſtilled over, I did not get half an ounce meaſure of air more than the veſſel contained; and this was dephlogiſticated. Conſequently, all the nitrous acid had been expelled from this nitre.

The colour of the alkaline matter to which the nitre was reduced in theſe earthen retorts, was a deep green, owing, perhaps, to ſomething of the earth in the retort; for in a porcelain veſſel, I, at one time, had it perfectly white.

At Mr. Watt's requeſt, I endeavoured to aſcertain the quantity of acid that was expelled from nitre, in procuring the dephlogiſticated air from it. To do this, I put two ounces of purified nitre into a glaſs retort, and receiving the air in 300 ounce meaſures of water, only filled each recipient half full, and agitated the air very much in the water, in order to make the water imbibe as much as poſſible of the acid that it contained. However, notwithſtanding this agitation, every veſſel of the air retained a ſtrong ſmell of the acid. The moment the air ceaſed to come, I filled a large phial with the water, and carried it to Mr. WATT, who carefully examined it; and in a paper which he preſented to the Royal Society, and which is publiſhed in the Philoſophical Tranſactions, he has given an account of the quantity of acid that it contained, computing from the ſpecimen that I brought him the quantity that was contained in all the 300 ounce meaſures of water; from which it may be fairly inferred, that there was no occaſion to ſuppoſe that any of the acid entered into the compoſition of the air, but that it was all either rendered volatile, or retained in the water.

I afterwards examined a portion of this water, by expelling air from it, and did not find it to contain more air than water generally does, but what I did get from it was conſiderably phlogiſticated.

SECTION XVIII.

Of the conſtituent Principles of Nitrous Air.

THERE is no kind of air, the conſtituent principles of which ſeemed to be more clearly aſcertained than thoſe of nitrous air, by thoſe philoſophers who admitted the doctrine of phlogiſton. We took it for granted, that it conſiſted of nitrous acid and phlogiſton. In an early period of my experiments I endeavoured to aſcertain the quantity of acid contained in it. Mr. KIRWAN has done the ſame, and Mr. CAVENDISH alſo has conſidered it as containing a very concentrated nitrous acid. I had no doubt on the ſubject till I read the work of Mr. METHERIE, who aſſerts that nitrous air contains no proper nitrous acid, but only one of the elements of it, the other being dephlogiſticated air, which had before been conſidered by Mr. LAVOISIER as the principle of all *acidity*.

Among other obſervations in ſupport of his aſſertion Mr. METHERIE has the following, Nitrous air burned together with inflammable air, produces no nitrous acid, p. 145. Though nitrous air is obtained from a ſolution of mercury in nitrous acid, almoſt all the acid is found in the ſolution, p. 146. Nitrous air abſorbed by marine acid, does not make aqua regia, p. 153. He is of opinion that a ſmall portion of the nitrous acid being decompoſed, furniſhes a pure air, ſo altered that, uniting with inflammable air, it changes it into nitrous air, p. 150.

As to this *hypotheſis* of Mr. METHERIE, I cannot ſay that I well underſtand it; but, being ſtruck with his *obſervations*, I began to review the experiments which I had formerly made on this kind of air, and could not recollect any of them in which it appeared that real nitrous acid was produced, except when nitrous air was combined with dephlogiſticated air, beſides the experiment in which it was decompoſed by the electric ſpark, which furniſhes a ſtrong objection to this hypotheſis. In every other view Mr. METHERIE's theory appeared to me not improbable, and led me to make the following experiments, which though not deciſively in favour of it, may deſerve conſideration. They will at leaſt ſhew that, if there be all the elements of nitrous acid in nitrous air, they muſt together with an additional portion of phlogiſton exiſt in the phlogiſticated air that is formed in the proceſſes.

When nitrous air is decompoſed by iron, or by a mixture of iron and ſulphur, I have obſerved that the water over which the proceſs is made, acquires no acidity by it; but I had ſuppoſed that all the acid had been ſeized by the *iron*. Having by me a conſiderable quantity of this iron which had been reduced to a perfect ruſt in nitrous air, and which I knew muſt have imbibed more than its weight of this air, I thought that I might get the acid out of it by diſtillation, if it contained any. But a quantity of this ruſt of iron, carefully diſtilled in an earthen retort, yielded neither nitrous air, nor nitrous acid, at leaſt in any quantity that could favour the common hypotheſis. For beſides the *air* (of which an account will be given in its proper place) ſome ounces of this ruſt yielded only a few drops of liquor, which was not ſenſibly acid to the taſte, and when poured into a little water, tinged blue with the juice of turnſole barely turned it red. But ſo ſlight a degree of acidity as this might have come (and I have no doubt but did come) from the union of nitrous air with thoſe bubbles of common air which could not be excluded from the veſſels containing the iron, in the many times in which I changed the air, when I ſupplied them from time to time with freſh nitrous air; ſo that the great maſs of nitrous acid which ought to have been depoſited in this iron muſt have been concealed in the phlogiſticated air, which is generally about one fourth of the original quantity of nitrous air. This brown ruſt (become ſo by expoſure to the air, which it imbibes) became black by the expulſion of its air and water, and exactly reſembled that iron ore which is found in the form of a black ſand.

I then endeavoured to decompoſe nitrous air by heating iron in it with a burning lens; and in this proceſs, I ſucceeded much beyond my expectation. For the air was preſently diminiſhed in quantity, while the iron became of a darker colour, was ſometimes melted into balls, and gained conſiderable weight. It had no appearance, however, of containing any nitrous acid, and yet water tinged with the juice of turnſole introduced into the veſſel, and agitated in it after the proceſs, never changed its colour.

In the firſt experiment the original quantity of nitrous air was diminiſhed to about one third, and after this it was increaſed. But then I found that when I had got to the extreme of diminution, there began to be an addition made of inflammable air from the iron; for this was evidently ſo. It was alſo obſervable that there was either ſome pure air, or dephlogiſticated nitrous air, in what remained of this proceſs. For when a meaſure of this reſiduum was mixed with an equal meaſure of common nitrous air, they were reduced to 1.7. I ſuſpect, however, that this was owing to a quantity of dephlogiſticated nitrous air being formed by this proceſs, as it is ſimilar to the ruſting of iron in nitrous air.

In another proceſs of this kind, I reduced ſix ounce meaſures of this air to three, and this was wholly phlogiſticated air, extinguiſhing a candle. I alſo heated iron in ſeven ounce meaſures and a half of nitrous air, till it was reduced to 3.7 meaſures, when it appeared to be perfectly phlogiſticated air. The iron had then gained 2¼ grains in weight, and at this time I obſerved that there was not the leaſt appearane of *moiſture* produced in the courſe of the experiment.

Then recollecting that, in my former experiments, Homberg's pyrophorus had taken fire in nitrous air, I made a quantity of this ſubſtance, and repeated the experiment with the greateſt care. But when nitrous air was decompoſed by this means, I ſaw no reaſon to think that any nitrous acid had been formed. In this proceſs the nitrous air is readily diminiſhed to about one-half of its bulk, after which nothing nitrous is found in it, but a portion of dephlogiſticated nitrous air; as appears by its being much more readily abſorbed by agitation in water than nitrous air is. But neither the air, nor any thing that can be expelled from the pyrophorus, diſcovers any ſign of containing nitrous acid.

Having ſaturated a quantity of pyrophorus with nitrous air in an earthen tube, which by this means became very hot, I admitted water to it; and diſtilling the pyrophorus with this water, the liquor that came from it would not change the colour of the juice of turnſole; and the air which came afterwards was of the ſame kind with that which is always expelled from pyrophorus after it has been burned in the open air, viz. partly fixed air, and partly inflammable.

I ſhall juſt mention ſome farther particulars of another experiment of this kind, though it was made long before I had the views which led to theſe now mentioned. Having ſaturated about two ounce meaſures of HOMBERG's pyrophorus with nitrous air, I obſerved that it had imbibed about four times its bulk of the air. This pyrophorus was of a kind that did not burn readily in the nitrous air, or I believe in common air, without external warmth. Having withdrawn it from the nitrous air, it took fire again, but with more external heat. Then putting it into an earthen retort, I expelled from it ninety ounce meaſures of air, a very ſmall proportion of which was fixed air. The former portions of it extinguiſhed a candle, and were of the ſtandard of 1.7, and the latter of 1.55, burning with a lambent blue flame. Three days after this, being taken out of the retort (the mouth of which had been all the time immerſed in mercury) a ſmall part of it took fire. I obſerved it then weighed 416 grains, and the next morning it weighed 438 grains.

There is ſomething very remarkable in this ſubſtance burning equally well in two ſuch very different kinds of air as nitrous and dephlogiſticated. It ſhould ſeem from this circumſtance, that there muſt be ſome common principle in the conſtitution of them both; though from their ſo readily uniting together, they ſeem to be things as different from each other as acid and alkali. This well deſerves a careful inveſtigation.

In my former experiments I found that the juice of turnſole became red by being impregnated with nitrous air. I found, however, that this muſt have been owing to ſome extraneous acid that was mixed with it. For when I took nitrous air that had ſtood long in water, and transferred it into a veſſel of mercury, I impregnated with it water tinged with the juice of turnſole without producing any change in its colour.

I have alſo obſerved that alkaline air admitted to nitrous air, made a whitiſh cloud in it. But this muſt have been owing to the ſame cauſe; the nitrous air in that experiment having been probably freſh made. For in the nitrous air which had been long confined by water, the mixture of alkaline air, did not produce the leaſt cloud. I perceived, however, that after the two kinds of air has been together a day or two, there was a ſmall quantity of a very deeply tinged blue liquor in the ſpace between the mercury and the glaſs; which ſhows that there was copper diſſolved in the nitrous air, and that it continued there after the extraneous acid was ſeparated from it. On this copper the volatile alkali had ſeized.

Notwithſtanding theſe reſults, when the electric ſpark was taken in the very ſame nitrous air, with which the preceding experiments were made, the juice of turnſole by which it was confined, preſently became red. This circumſtance deſerves to be examined with care, as it ſeems to be deciſively in favour of the nitrous air containing all the neceſſary elements of nitrous acid. An account of ſome experiments of this kind will be found in the next ſection.

SECTION XIX.

Of Nitrous Air in general.

THE experiments that I have lately made on the ſubject of nitrous air, relate chiefly to that modification of it which, from its property of admitting a candle to burn in it, I have denominated *dephlogiſticated nitrous air*; but before I proceed to theſe, I ſhall, in this ſection, recite a few others of a miſcellaneous nature.

Water ſeems to be a neceſſary ingredient in nitrous as well as in inflammable air, at leaſt without a quantity of water, nitrous air cannot be formed. For example, copper will be diſſolved in ſtrong nitrous acid, but without producing any air, juſt as inflammable air is not produced unleſs the vitriolic acid in which the iron is diſſolved be much diluted with water.

That nothing is neceſſary to the formation of nitrous air beſides phlogiſticated nitrous acid and water, is evident from the production of it by the impregnation of pure water with phlogiſticated nitrous vapour, formed by the rapid ſolution of biſmuth; an experiment which I have mentioned before. However, to make it in a more unexceptionable manner, I interpoſed a glaſs veſſel between that in which the ſolution was made, and that in which the water to be impregnated with the phlogiſticated vapour was contained; that whatever of the ſolution was diſtilled over by the heat of the proceſs (which evidently occaſioned the dropping of a blue liquor from the end of the tube through which the vapour was delivered) might be prevented from reaching the water. In theſe circumſtances, however, when nothing but the dry phlogiſticated vapour could enter the water, it began to ſparkle, and yield nitrous air very copiouſly, as ſoon as it had acquired a blue tinge from the impregnation. This is a very ſtriking experiment to all perſons when they have ſeen it the firſt time.

Nitrous air is alſo produced by pouring a highly coloured, or phlogiſticated, nitrous acid into pure water, in which no metal or earthy matter is any way concerned.

I have formerly obſerved, that a ſolution of green vitriol readily decompoſes nitrous air, and thereby becomes very black, but that if this black ſolution be expoſed to the air, it loſes its colour again. Having impregnated a quantity of the ſolution in this manner, I heated it in a glaſs phial, and found that it gave out half its bulk of pure nitrous air, but when it had loſt its dark colour, it gave no air at all by heat.

I have likewiſe obſerved how readily nitrous air is diminiſhed by taking the electric park in it. This experiment I have frequently repeated, in order to aſcertain more particularly the quantity and quality of the reſiduum. In one experiment I reduced half an ounce meaſure of nitrous air, in leſs than half an hour, to one quarter of its bulk; but then one-fourth of it was ſtill nitrous, and the reſt phlogiſticated air. Receiving it in lime water, there ſeemed to be a ſlight precipitation; but this circumſtance will perhaps be explained in the following experiment.

I took the electric ſpark in a quantity of nitrous air till there remained no more than one-third of it, and this was completely phlogiſticated, not affecting common air at all, and extinguiſhing a candle. A white matter was formed with the mercury over which the ſpark was taken, which made the water admitted to it extremely turbid. At that time I ſuppoſed that this white ſubſtance was mercurial nitre, conſiſting of nitrous acid from the decompoſed nitrous air and mercury. I ſhall now examine it more particularly. Why there ſhould be more than one-third of this quantity of nitrous air left pure phlogiſticated air, and not one-fourth, as in the former experiment, I cannot tell.

In another proceſs I took the electric ſpark in a quantity of nitrous air, till it could be no more diminiſhed; when it was in proportion to its former bulk, as 10½ to 24. Letting it ſtand all night upon the mercury, it was increaſed to the proportion of 11¼ to 24, ſeemingly by the acid uniting to the mercury, and generating more nitrous air, ſince it had that ſmell. No water appeared after the proceſs, and the water admitted to it acquired no acid taſte, but an aſtringent one, like water impregnated with nitrous air. There was a white powder formed, as in the former experiments.

Thinking to make water imbibe the acid from the nitrous air, decompoſed in this proceſs, I took the electric ſpark in it with a ſmall quantity of water over the mercury. But even this water did not acquire any acid taſte, but only an aſtringent one.

It remains to be aſcertained on what principles the nitrous air is decompoſed in this proceſs. That it is not by means of the *heat* communicated to it, may, I think, be inferred from an experiment, in which I made a quantity of nitrous air paſs through a red-hot tobacco pipe, without producing any change in it. Neither does it ſeem to be the *light* of the electric ſpark, ſince I have heated pieces of crucibles in nitrous air by the ſun beams, without making any change in it.

Upon the whole, it may be moſt probable that this change depends upon *phlogiſton*, as it is ſimilar to that which is made in nitrous air by the contact of iron, and eſpecially by heating iron in this air. There is this difference, however, in the two proceſſes; that in that with electricity the reſiduum is wholly phlogiſticated air, and the intermediate ſtate of dephlogiſticated nitrous air is not found. At leaſt I have not obſerved it.

SECTION XX.

Experiments relating to Dephlogiſticated Nitrous Air.

BEFORE I recite the experiments which I have made on this kind of air, I ſhall mention ſome obſervations on the proceſs by which it is procured. And the firſt thing that I ſhall obſerve is, that *water* is abſolutely neceſſary to this decompoſition of nitrous air, when it is effected by iron. I had decompoſed it before, either by previouſly filling the veſſels that were to contain the nitrous air with *water*, or with *mercury*, though it had always required much longer time when mercury was uſed. The reaſon of this difference I did not then know; but I now perceive that it was owing to a ſmall quantity of *moiſture* in the veſſel containing the mercury.

Suſpecting the influence of *water* in this proceſs, I procured a number of very clean ſmall needles, and having made a phial, and alſo a proper quantity of mercury, perfectly clean and dry, I put the needles into the phial; and filling it up with mercury, introduced the nitrous air. This phial continued in the ſame ſtate ſix or eight months, without the leaſt ſenſible change in the ſpace occupied by the air. I therefore concluded that no change had taken place in it, and therefore introduced a few drops of water; after which I perceived, in a day or two, that the air was diminiſhed, and the diminution proceeded till about one-third of the air had diſappeared. In this ſtate the phial continued ſeveral months more; when, obſerving no farther change, I examined it, and found that a candle would not burn in it, which I had expected it would have done; as I imagined there was not water ſufficient to imbibe that part of the air which had enabled a candle to burn in it; and how this part of the air ſhould diſappear, or be converted into phlogiſticated air, without water, I could not tell. In another experiment the reſult was ſeemingly different.

On the 26th of May, 1782, I examined a quantity of nitrous air, which had been confined by mercury with iron ſhavings from the 27th of Auguſt preceding, when I found one-half of it abſorbed, and a candle burned in the remainder better than in common air, though a mouſe died in it; and yet this air had remained ſeveral months in the ſame ſtate, with reſpect to quantity, in which it was when I examined it; ſo that I concluded it would never have extinguiſhed a candle, in conſequence of any longer continuance in that ſtate. In this caſe, however, there was no ſenſible quantity of water at all; whereas in the other there were a few drops. Water, therefore, may contribute to this change in the air, independently of its *abſorbing* any part of it. When I agitated this air in water, ſomething more than one-half was abſorbed, and the remainder extinguiſhed a candle.

But, perhaps, the more probable inference from this experiment may be that, in time, all the dephlogiſticated nitrous air is imbibed by the iron, ſo that nothing beſides the phlogiſticated air will be left in the veſſel; though there was no appearance of this being the caſe in the experiment with the needles; as the air did not continue to diminiſh longer than uſual, and there remained two-thirds of the original quantity of phlogiſticated air. This, therefore, rather looks like a change of the dephlogiſticated nitrous air into phlogiſticated air, by the addition of phlogiſton from the iron.

Another experiment I made with a view to aſcertain the degree of diminution at which the nitrous air begins to be decompoſed. On the 14th of May, I put twenty-four ounce meaſures of nitrous air into a quart bottle, previouſly filled with pieces of iron, that had before been ruſted in this proceſs. On the 18th there were only ſeventeen ounce meaſures of air, and it was not at all changed; on the 21ſt there were thirteen ounce meaſures, and on the 31ſt four ounce meaſures, of which half was then abſorbed by water, and the remainder was phlogiſticated air. This was the greateſt diminution that I remember to have obſerved in this proceſs. But having neglected it from the 21ſt to the 31ſt, I did not find the time at which the decompoſition firſt took place; but in general I believe it is when about one-fourth of the quantity is remaining, and that the change takes place pretty ſuddenly, for I do not remember ever to have found it in a middle ſtate.

Having a quantity of iron that was reduced to a perfect *ruſt* in theſe proceſſes, I had the curioſity to heat it in an earthen crucible, in order to examine the *air* that might be expelled from it, and the reſult was as follows. From ten ounces of this ruſt, I got about 180 ounce meaſures of air, mixed at firſt with much ſteam. The firſt ſixty ounce meaſures were chiefly fixed air, with a phlogiſticated reſiduum, of the ſtandard of 1.7. The remainder came much more ſlowly, requiring about four hours continuance of heat. This contained no mixture of fixed air, and was ſo pure, that a candle burned in it pretty well, being of the ſtandard of 1.32. The matter was reduced to one maſs, like iron, ſtrongly attached by the magnet. But, indeed, the ruſt itſelf had been ſo before the proceſs.

It ſeems probable, that the dephlogiſticated nitrous vapour is contained in this ruſt, and that with the addition of the principle of heat it becomes proper dephlogiſticated air.

In order to compare this kind of ruſt of iron with that which is formed by expoſure to the atmoſphere, I took four ounces of the latter; and, by the ſame treatment, got from it 180 ounce meaſures of air, about one-half of which was fixed air, with a reſiduum of the ſtandard of 1.7; but the bulk of it was inflammable, burning with a lambent blue flame.

Dephlogiſticated nitrous air is the term by which I firſt diſtinguiſhed this ſpecies of air, becauſe it admitted a candle to burn in it (as I had called that air *phlogiſticated* in which a candle would not burn, and that *dephlogiſticated* in which it burns better than in common air, which is alſo the caſe with that ſpecies of air of which I am now treating) and I now think I ſee more reaſon to approve of the appellation. Though this air agrees with dephlogiſticated air in admitting a candle to burn in it, and ſometimes with peculiar brilliancy, it is as fatal to animals as the moſt perfectly phlogiſticated air. As an attention to the other different properties of theſe two kinds of air, which agree ſo remarkably in one circumſtance, may be a means of throwing ſome farther light on the nature of *combuſtion* and *reſpiration*, I ſhall proceed to recite the facts which I have lately obſerved relating to it.

I have formerly produced this dephlogiſticated nitrous air by the contact of nitrous air with iron, and I have not yet completed my obſervations on this particular proceſs, ſo as to have aſcertained the quantity of nitrous air that a given quantity of iron can decompoſe, the quantity of iron that a given quantity of nitrous air can deſtroy, &c. As yet I have ſeen no iron ſo far affected by this proceſs, though it is in a ſtate that eaſily crumbles to powder, as not to be able to decompoſe more nitrous air, and almoſt as readily as freſh iron.

I ſhall juſt mention one experiment in which this ruſted iron, and freſh iron, were made uſe of. The nitrous air had been put to each of them on the 27th of Auguſt, and they were both examined on the 18th of October; when, in both caſes, the air was found to be diminiſhed to about one third, and a candle burned with equal brilliancy in both. Water then abſorbed about one half of each of them, and the remainder extinguiſhed a candle. Neither of them had in the ſmalleſt degree the property of freſh nitrous air.

This may ſerve as a recapitulation of my former obſervations on this ſubject, and at the ſame time ſerve to ſhew the proper generation of phlogiſticated air from nitrous air and phlogiſton, agreeably to that moſt valuable diſcovery of Mr. CAVENDISH, who has actually produced nitrous acid from phlogiſticated air. Here we ſee that nearly one fourth part of any quantity of nitrous air may be converted into phlogiſticated air; and as the nitrous acid vapour unites with the iron, it is probable that all the phlogiſton in the nitrous air, and perhaps alſo a part of that which belonged to the iron itſelf, may be employed in the formation of phlogiſticated air. For ſince ſome pure air is expelled by heat from the iron that has been uſed in this proceſs, it is poſſible that it had been deprived of ſome of its phlogiſton. But more *data* may be wanted to determine, from this proceſs, the proportion of phlogiſton in phlogiſticated air.

This proceſs for procuring dephlogiſticated nitrous air requiring much time, I have of late had recourſe to another, which has been to diſſolve turnings of iron in a dilute ſolution of copper in the nitrous acid (the ſame that remains from making nitrous air) mixing with it again about an equal quantity of water. Without this precaution, though the iron will at firſt be acted upon very ſlowly, yet the mixture will at length grow ſo hot, as actually to boil, and the proceſs will be exceedingly troubleſome. It will be neceſſary, however, previouſly to heat the ſolution of copper, in order to expel from it all ſuperfluous nitrous acid, though the keeping in it pieces of copper might anſwer the ſame purpoſe. Without one or other of theſe precautions, part of the air procured by diſſolving iron in it will be proper nitrous air; but with this precaution the whole of it will be ſuch as nitrous air is reduced to by expoſing common nitrous air to iron, that is, one third of it will generally be abſorbed by water, and the remainder will be proper phlogiſticated air. And having this mixture of dephlogiſticated nitrous air and phlogiſticated air, the former will be procured pure by agitating the mixture in water, which will imbibe it, and leave the phlogiſticated part unabſorbed.

A given quantity of water, I find, will take up about one half of its bulk of this dephlogiſticated nitrous air; and it does it almoſt as readily as water imbibes fixed air. The water thus ſaturated with the air muſt be heated, and then the dephlogiſticated nitrous air will be expelled pure, and may be received in veſſels containing mercury. I obſerved, however, that as this kind of air much reſembles fixed air, in its properties of being imbibed by water, and being expelled from it again by heat, it does ſo alſo in this farther property, that all the air which has been actually incorporated with the water will not be imbibed by water again. But the proportion of this part is much greater in this kind of air than in fixed air. For in this caſe I have found it to be as 1 to 10; whereas in fixed air it is as about 1 to 30, or 40. This reſiduum being examined, was found to be air partially phlogiſticated, the ſtandard of it being about 1.4, ſo that a candle will juſt burn in it; but the quality of this reſiduum is not always the ſame. The reſiduum of fixed air is generally much purer, viz. of the ſtandard of 1.0.

I obſerved, that when water fully impregnated with this dephlogiſticated nitrous air is expoſed to the atmoſphere, it very ſoon parts with it, much ſooner than water impregnated with fixed air.

I mixed dephlogiſticated nitrous air with all the other kinds of air that I have made, but it did not unite with any of them; and no ſenſible change was made in *it* by any of them, or by any of them in it; except that dephlogiſticated air was injured, being reduced from the ſtandard of 0.3 to 0.8. But this was, no doubt, by the addition of that part of this air which is not miſcible with water, and is conſiderably phlogiſticated.

In this pure kind of dephlogiſticated nitrous air a candle burned better than in common air, but not ſo well as I have frequently known it to do, as it is originally produced mixed with phlogiſticated air; which ſeems to ſhew that ſome change is made in it by being incorporated with water. When it was mixed with inflammable air, they were exploded together with violence, very much like a mixture of inflammable and dephlogiſticated air; but a mouſe died in it as quickly as in fixed air.

It was natural to ſuſpect, that there might be ſomething *acid* in this air. But though it was readily abſorbed by water tinged blue with the juice of turnſole, it made no change in its colour. Neither was there any thing alkaline in it. For when the ſlighteſt tinge of red was given to this liquor by an acid, the blue colour was never in the leaſt degree reſtored by its impregnation with this kind of air.

The ſame thing may indeed be inferred from its not incorporating with either acid or alkaline air. In the experiment with the latter it appeared that this dephlogiſticated nitrous air is lighter than common air. For mixing it with alkaline air, and then putting acid air to it, the white cloud formed by their union was almoſt wholly in the lower part of the veſſel. But it is not ſo much lighter as inflammable air is found to be by the ſame experiment. For in this caſe the white cloud never reaches the top of the veſſel; where the inflammable air appears to remain, intirely unmixed with the alkaline air.

Thinking that, though dephlogiſticated air ready formed might not affect, or be affected by dephlogiſticated nitrous air, it poſſibly might in its firſt formation; to try this, I revived mercury from red precipitate in this air. But the operation proceeded juſt as it would have done in vacuo, the mercury being revived very readily, and dephlogiſticated air produced, without affecting, or being affected by, the dephlogiſticated nitrous air.

No leſs remarkable was the reſult of heating malleable iron in this air. For the bulk of the air was increaſed, and yet afterwards no part of it was imbibed by water. It was all phlogiſticated air, without any mixture of fixed air. It ſeems, therefore, as if this kind of air wanted nothing but phlogiſton to make it phlogiſticated air, and nothing but the principle of *heat*, to make it dephlogiſticated air.

This experiment happening to draw my particular attention, I repeated it very often, and always had the ſame reſult, though with variations, depending on the quantity of the air, and the heat I was able to apply. If this was ſufficient to melt the bits of iron, the effect was produced very quickly. As the particulars may be of uſe to future experimenters, I ſhall recite ſome of them.

Of three ounce meaſures and a half of dephlogiſticated nitrous air, two ounce meaſures remained unabſorbed by water after heating malleable iron in it. During the proceſs, the veſſel had been full of whitiſh fumes. What remained of the air was of the ſtandard of 1.7, and extinguiſhed a candle.

Throwing the focus of the lens about five minutes upon ſome filings of iron in this kind of air, the quantity was increaſed one tenth. Afterwards water abſorbed about one ſixth of it, and the remainder extinquiſhed a candle, being of the ſtandard of 1.8.

I repeated the experiment with caſt iron turnings, and had the ſame reſult. About one fifth was added to the quantity, water abſorbed about the ſame proportion of it, and the remainder was of the ſtandard of 1.85. Some little common air may be ſuppoſed to have been introduced into the veſſel along with the turnings or ſhavings of iron, but this could not be much; and in all theſe experiments the addition produced in the proceſs was a very ſmall part of what remained unabſorbed by water; ſo that there can be no doubt but that this operation produces a real change in the quality of the air, rendering it almoſt wholly phlogiſticated.

In order to produce this effect, the heat muſt be very conſiderable, or the pieces of iron very ſmall, ſo that they muſt at leaſt be red hot. For when I uſed whole caſt iron nails, on which I ſuppoſe the heat of my lens could not make ſufficient impreſſion, the effect of applying it an hour and a half, and with a very good ſun, was very inconſiderable; the proportion of that part of the air which was immiſcible with water being only increaſed in the proportion of 50 to 27.

But the circumſtance to which I happened to give the greateſt attention with reſpect to this kind of air, in the courſe of the laſt ſummer, was the change that is made in it by *heat*, or *light*. For ſuch muſt be the agents, when bits of clean crucibles, or retorts, are heated by a burning lens in any kind of air. The conſequence of this proceſs always was both an increaſe of that part of the air which will not unite with water, and alſo rendering it purer than before, approaching nearer to the ſtate of atmoſpherical air; exactly like the reſult of taking the electric ſpark in fixed air. I frequently ſtopped in the middle of this proceſs, and always found theſe effects to be more conſiderable the longer I continued it. I ſhall give the particulars of thoſe experiments only, on which I place the greateſt dependence.

Having impregnated a quantity of boiled water with dephlogiſticated nitrous air, I found that the air expelled from it was again abſorbed by water, in the proportion of 0.7 to 10.1, and the ſtandard of this reſiduum was 1.7. I then heated in it pieces of crucibles which had been juſt before made red hot, introducing them into the air by means of a forceps; and after heating them an hour and a half, in a very hot ſun, found that a portion of the air was then unabſorbed by water in the proportion of 4 to 7, and the ſtandard of the reſiduum was 1.55. I reſumed the experiment with what remained of the air, and continued it half an hour longer, when the proportion of the part that was not abſorbed by water, was to the other as 26 to 40, and the ſtandard of this reſiduum was 1.45. No moiſture ever appeared in the inſide of the veſſel, and there was no ſenſible change in the quantity of the air.

In a portion of the ſame dephlogiſticated nitrous air that was uſed in the former experiment, I heated bits of crucibles, which, after being dipped in water, had only been dried, though they had been made very hot in the ſun. In the courſe of this proceſs, much water appeared in the veſſel, expelled no doubt from the earthen ware; and after the proceſs had been continued an hour and a half, the part of the air that was unabſorbed by water, was to the reſt as 4.5 to 7, and the ſtandard of it was 1.45.

Examining a quantity of dephlogiſticated nitrous air, in a portion of which I was about to heat bits of dry crucibles over mercury, I found that by agitation in water 80.5 meaſures of it was reduced to 3.5 meaſures, and the ſtandard of this reſiduum was 1.58. But of that portion of the ſame air in which bits of crucibles had been heated about an hour in all, 63 meaſures were only reduced to 21, and the ſtandard of this reſiduum was 1.38. On this occaſion I obſerved that the inſide of the glaſs veſſel was pretty thickly coated with a black matter. Indeed, in the former experiments, it had always acquired a dark incruſtation; but I did not know whether it might not have come from the earthen ware. I think it highly probable, that it was mercury ſuper-phlogiſticated, as every other black incruſtation in proceſſes in which mercury was uſed has been. It is probable, however, that ſome phlogiſticated matter paſſed from the air to the mercury, which would of courſe render the reſiduum more phlogiſticated, though I do not perceive how it ſhould follow that it ſhould thereby render it leſs miſcible with water. But whatever proportion of phlogiſton be neceſſary to make any kind of air miſcible with water, any *change* in that proportion may make it leſs miſcible with it.

In theſe reſpects this change in the conſtitution of dephlogiſticated nitrous air, very much reſembles that which is made in fixed air, both by the electric ſpark, and alſo by heating bits of crucibles in it; an account of which has been given before.

I endeavoured to aſcertain whether this change in the conſtitution of dephlogiſtidated nitrous air was produced by mere *heat*, without light, by heating the air in earthen tubes or retorts, when their mouths were immerſed in mercury; but, to my great diſappointment, I found that all kinds of air heated in this manner, came out common air, or rather air a little phlogiſticated. In fact, the earthen veſſels, in that degree of heat to which I expoſed them, muſt have been rendered pervious to air, though they were glazed inſide and outſide, and were perfectly air tight both before and after the experiment.

But the moſt effectual change that I was able to make in this kind of air, was bringing it to a ſtate in which I doubt not it was reſpirable, by means of the electric ſpark. Taking this ſpark, or making ſmall exploſions, in 0.4 of an ounce meaſure of dephlogiſticated nitrous air, it was rendered wholly immiſcible with water, and of the ſtandard of 1.45.

From this experiment it ſhould ſeem, that there is in this ſpecies of air ſomething that is capable of being converted into pure air. This, indeed, is ſufficiently indicated both by a candle burning perfectly well in it, and alſo by pyrophorus ſpontaneouſly kindling in the nitrous air from which it is formed. A judicious proſecution of theſe experiments may therefore throw light upon the nature of both the kinds of air, and of combuſtion alſo. This kind of air, however, though a candle burns ſo well in it, will not kindle pyrophorus. I put a ſmall cupful of the pyrophorus into a quantity of this kind of air, and let it remain there a conſiderable time, without perceiving any ſenſible effect; but when I took it out, and put it into a veſſel containing nitrous air, it took fire immediately.

In the courſe of theſe experiments with the ſun, I obſerved a remarkable ſource of fallacy, with reſpect to the increaſe of the quantity of air confined by mercury, when there is ſo much moiſture in the inſide as to be ſubject to ſudden dilations and compreſſions. For a conſiderable quantity of common air would get into the inſide of the veſſel, when there was the depth of an inch of mercury on the outſide of it, and of two or three inches within. In theſe circumſtances I have ſeen more than an ounce meaſure of the external air gain admiſſion in leſs than one minute. This muſt have been occaſioned by the mercury never being in perfect contact with glaſs; ſo that when the mercury was in a ſtate of undulation, the air that was confined between it and the glaſs was continually protruded, and more air from the atmoſphere was forced into its place, by the ſame preſſure which ſupported the column of mercury within the glaſs. This effect I prevented by having a quantity of water upon the mercury, on the outſide of the veſſel. For this would be in perfect contact with the glaſs; and in this caſe I never found either air or water to get into the veſſel to diſturb my experiment.

SECTION XXI.

Experiments with Electricity.

THE application of electricity to chymiſtry is as yet in its infancy. I have made ſome uſe of it in the doctrine of air, and Mr. CAVENDISH has lately made a moſt capital one. This ſucceſs ſhould encourage us to apply to it with more aſſiduity.

One of my firſt experiments on the ſubject of air was that by which I procured inflammable air by taking the electric ſpark in different kinds of oil. In the *acids* no ſpark can be taken, on account of their being ſuch excellent conductors of electricity. However, a ſmall bubble of air may be left confined by any acid, and in this a ſpark may be taken, which will affect the contiguous fluid. This I have done, and ſome of the reſults are ſufficiently remarkable; but ſimilar to the effects of tranſmitting the liquors in vapour, through a red hot earthen tube, in which caſe the nitrous and vitriolic acids yield dephlogiſticated air.

In order to take the electric ſpark in *nitrous acid,* I admitted a ſmall bubble of common air into a glaſs ſyphon, previouſly filled with that acid, placing each leg of the ſyphon in a different veſſel, containing ſome of the ſame acid, and I tranſmitted the ſpark, or ſhock, through a gold wire, on which the acid has no action. In theſe circumſtances every ſpark made a conſiderable addition to the quantity of air, and it appeared to be the pureſt dephlogiſticated air.

With the *vitriolic acid* I had a ſimilar reſult, but much more time was required to produce the effect. After taking the ſpark two hours in a bubble of air confined by this acid, in a ſyphon, the greateſt part of which was filled with mercury, and the legs of it ſtanding in baſons of mercury, the bubble was not more than doubled in quantity; but the addition that was made to it appeared to be dephlogiſticated air, the ſtandard of it being, with equal quantities of nitrous air, 1.0. It may be worth noticing, that though the oil of vitriol, uſed in this experiment, was of a light purple colour (which might be the reaſon why the the air procured from it was not both more in quantity, and better in quality) the part next to the air became perfectly colourleſs for the ſpace of about a quarter of an inch.

When I repeated this experiment with common *ſpirit of ſalt*, the bubble of air was diminiſhed one fourth; and though the operation was continued a long time, it never increaſed afterwards, and was ſimply phlogiſticated air. When I uſed dephlogiſticated marine acid, the bubble of air was diminiſhed near one-half, but what remained was ſtill phlogiſticated air.

With *phoſphoric acid*, the air was firſt diminiſhed one-quarter, and then increaſed. This air I could perceive not to be affected by nitrous air; and by the redneſs of the electric ſpark in it, I have no doubt of its being inflammable air. But the quantity was too ſmall to aſcertain it in any other manner. This acid is always ſaid to contain phlogiſton, and by this means it ſeems to be converted into inflammable air.

I had a ſimilar reſult when I took the electric ſpark in air confined by *phlogiſticated alkali.* It was firſt diminiſhed one-fourth, and then increaſed to its original bulk; but the increaſe was very ſlow. When I examined the air, I could only obſerve that it was not affected by nitrous air. The addition muſt, I think, have been inflammable air, though the quantity was ſo ſmall, that the ſpark had no ſenſible redneſs in it.

I ſhall conclude the account of theſe experiments with mentioning one of ſmall conſequence, relating to the conducting power of ſubſtances. Having boiled ſome linſeed oil, I put into it a quantity of pounded glaſs. When it was cold it was a ſolid black maſs, and conducted electricity perfectly well through its whole ſubſtance, and was not, as I had expected, that kind of ſubſtance which conducts electricity *in a viſible manner*, ſuch as I have deſcribed in my obſervations on charcoal, and which I had often found to incruſt pieces of glaſs that had been made red hot in vegetable oils. I then melted the whole maſs; but though it remained black throughout, and had been covered from all acceſs of the external air, it had no more conducting power than any other kind of glaſs.

SECTION XXII.

Of the Influence of Light on Vapour of Spirit of Nitre.

LIGHT, beſides ſerving the important purpoſe of *viſion*, is likewiſe a chymical principle, the effects of which are as yet but little known; though we have ſeen ſufficient reaſon to conclude, that it is a very important agent in the ſyſtem of nature.

Any new facts, therefore, relating to ſo curious a ſubject, muſt be acceptable to the natural philoſopher.

After the experiments which I had before made on the colouring of ſpirit of nitre, by which it appeared to depend upon phlogiſton, it was ſuggeſted to me by a philoſophical friend, that the *air* incumbent upon the acid might poſſibly affect its colour, and I was deſired to attend to that circumſtance. Accordingly I did ſo, and found that air *as ſuch* had no influence in the caſe, but a very material one, as affording ſpace for the vapour of the acid to expand itſelf in; ſo as to be, in that ſtate, ſubject to the action of *light*, a thing of which I had no ſuſpicion before.

Having made a quantity of colourleſs ſpirit of nitre, which is readily done by making it boil haſtily (in order to prevent too great a loſs of the acid) and letting it cool again in the dark, I put different portions of it into ſeveral phials, ſome of them quite full, and others only half full, with every different ſpecies of air incumbent upon them, except the nitrous; which I knew would immediately be decompoſed, and give it colour. Then leaving the phials expoſed to the light of the ſun, in a few days I found the acid in all of them that were only *half full* conſiderably coloured; whereas, the acid in the phials that were quite full remained as colourleſs as water.

After having had air of different kinds in thoſe phials which were only partially filled; I contrived to have a *vacuum* above the acid; but ſtill, when it was expoſed to the light, it became coloured, as well as that which had air in contact with it*.

I then took ſome of the phials that were only *half full*, and covering them from the light expoſed them for ſeveral days to a conſiderable degree of *heat*. But in that ſituation they never acquired any colour.

Being now ſatisfied that it was the action of *light* upon the *vapour* of ſpirit of nitre that gave it colour, I amuſed myſelf with throwing a ſtrong light, by means of a lens, into the upper part of a phial, the lower part of which contained colourleſs ſpirit of nitre. And in this manner I found that I could ſoon give a ſtrong orange colour to the vapour of the acid; and that, being imbibed by the liquid acid with which it was in contact, *this* alſo became coloured, firſt at the top, and then quite through its ſubſtance.

---

* In this experiment I uſed a glaſs transferrer, executed by Mr. Parker. It is of excellent uſe when either acids or mercury is employed.

In what manner light acts, in order to give colour to the vapour of nitrous acid, remains to be determined; but that it is by imparting *phlogiſton* to it, is highly probable, becauſe the liquid acid will become coloured by means of any thing that contains phlogiſton, independent of light. It remains alſo to be aſcertained, why light has not a ſimilar effect on the vapour of ſpirit of ſalt, that of other acids, and vapours of other kinds. But when a ſufficient degree of attention ſhall be excited to the ſubject, it may not, perhaps, be very difficult to make much farther progreſs in the inveſtigation of the chymical properties of light, and its action upon bodies in general.

I had been told that the blue colour of the ſolution of copper in ſpirit of nitre depended upon light. But when I was engaged in the preceding experiments, I ſatiſfied myſelf that it did not. For having firſt taken the moſt effectual care that the ſolution was made with the moſt colourleſs acid, I examined it afterwards in the night, with the fainteſt light of a candle; and in ſeveral trials always found it as blue as any ſolution that had been made in the open day-light. I therefore conclude that phlogiſton, *as ſuch*, independent of any thing elſe relating to light, is the thing that gives colour to this acid. That light does contain phlogiſton, is, I think, evident from various chymical experiments.

SECTION XXIII.

Experiments on the Action of Steam upon ſeveral heated Subſtances.

HAVING, ſince the publication of that paper in the Philoſophical Tranſactions which makes the ſecond ſection of this volume, made a few other experiments of the ſame kind with thoſe recited in it, I ſhall introduce them here, by way of *Supplement* to it. Thoſe already recited relate chiefly to the action of ſteam upon *charcoal*, or *iron*. I have ſince extended them to ſome other ſubſtances, and though the reſults are not very important, it may be of ſome uſe to relate them.

Having made another experiment with *iron*, with as much attention as I could give to it, I ſhall in the firſt place mention *that*. From two ounces of iron turnings (which is caſt iron annealed) I got in the firſt inſtance 985 ounce meaſures of air, with the loſs of 528 grains of water; and the iron, I found, had gained 292 grains in weight. Then making four ounces of water of this reſiduum paſs over the ſame iron, it gained four grains more, and all the air that I procured was 998 ounce meaſures. So that (as extreme accuracy is not to be obtained in proceſſes of this kind) it may be ſaid that two ounces of this kind of iron will, in this way, yield 1000 ounce meaſures of air; whereas by ſolution in vitriolic acid it would have yielded about 800. Conſequently more air by two fifths may be procured by this new mode of treatment, which alone ſhould recommend it to thoſe who fill balloons.

Having recommended this proceſs as the cheapeſt and the moſt convenient for filling balloons, eſpecially when tubes of caſt iron ſhould be made uſe of; I was willing to make a trial of one, in order to form ſome judgment how long they would laſt for the purpoſe. I therefore procured one of an inch and a quarter in diameter, and not more than a quarter of an inch in the thickneſs of the metal; and making the middle part of it red-hot, ſent ſteam through it; and from the reſult of four or five proceſſes with the ſame tube, I have little doubt, but that, if they were made of the thickneſs of half an inch in the metal, and care was taken to coat them on the outſide with clay and ſand, the ſame tube might probably ſerve twenty times. That the reader may form ſome judgment as well as myſelf, I ſhall mention the reſult of my obſervations.

I heated the tube four or five different times, and in each proceſs tranſmitted as much water through it as would have been more than ſufficient to decompoſe all the iron that it could have contained. At firſt four ounce meaſures of water procured 180 ounce meaſures of inflammable air, and then ſix ounce meaſures procured only 160 ounce meaſures. I then examined the tube, and found that when both the inſide and outſide were well ſcraped with a ſharp inſtrument, it had loſt twenty-five grains in weight. The ſcales from the outſide weighed 282 grains, while all that I could get from the inſide weighed only 36 grains. Conſequently the tube had gained in weight 283 grains. After this I heated it again, and tranſmitted through it ſix more ounces of water, which yielded only 60 ounce meaſures of air.

From theſe experiments it may be inferred, that the tube would ſoon have ceaſed to give any air; the inſide being changed into ſome depth by the action of the ſteam, and yet it was not much diſpoſed to exfoliate. In time it would, no doubt, have become brittle, and might be in danger of breaking, from its diſpoſition to *bend* in the courſe of the proceſs. This bending was very conſiderable; but did not ſeem to ariſe from any tendency in the iron to *melt*. Perhaps by turning it in cooling, this bending, and conſequently the danger of cracking after much uſe, might be prevented; or this property of bending might in a great meaſure ceaſe, when the metallic ſtate of the tube was deſtroyed; and yet with care might continue a firm and compact tube, and as fit for this proceſs as at the firſt. If this ſhould be the caſe (which experience alone can determine) it is not to ſay how long a tube of this kind might laſt. It would then be a kind of *earthen tube*, of the moſt perfect nature, completely air tight, without being ſubject to ruſt or decay.

Upon the whole, ſhould the fondneſs for balloons be reſumed, I ſee no reaſon why far the greateſt part of the expence attending the filling of them might not be ſaved by means of this proceſs. A complete apparatus for it would not coſt half ſo much as the filling of a ſingle balloon, that would carry a man, in the common way, and would ſerve at leaſt a conſiderable number of times, with the expence of a very few pounds each time; as there would be hardly any thing to pay for beſides *fire*, and *attendance*, for a few hours. For ſuch *iron* as would beſt anſwer for this purpoſe might, in moſt places, be had for a mere trifle. One apparatus, conveniently fixed, might ſerve for a whole town or neighbourhood.

Having been able to decompoſe iron by converting it into ſcales, I alſo found that in this way, I could readily procure *flowers of zinc*, as well as inflammable air from that metal. The flowers came over in a very attenuated ſtate, the air being loaded with them. It might be poſſible, however, to contrive an apparatus to collect them.

Braſs being made with a mixture of zinc and copper, and *zinc* being ſo eaſily decompoſed by ſteam, whenever a copper tube is ordered for the purpoſe of theſe experiments, particular care ſhould be taken that there be no mixture of braſs in it, though it is difficult to have copper caſt ſmooth and ſolid without a mixture of either braſs or tin, which is not much better. Even pure copper tubes become brittle, and at length crack in theſe experiments.

Having at one time been perſuaded to have a little braſs mixed with the copper in one of theſe tubes, I conſented that the ſmalleſt quantity that could be ſuppoſed to be neceſſary to make the tube compact, ſhould be put into it. But notwithſtanding this, and though the tube had a quarter of an inch thickneſs of metal, it fell to pieces in the very firſt experiment, in which I ſent the ſteam of no more than two or three ounces of water through it. Inflammable air was produced very copiouſly, and the flowers of zinc were mixed with it; ſo readily did the ſteam ſeparate the zinc from the copper, though the heat was only ſufficient to make the tube red hot, and was far from melting it.

Lead would probably be far leſs affected than copper in theſe experiments; but then it will not bear a read heat without melting. I made the ſteam of about four ounces of water paſs over four ounces of melted lead, in an earthen tube, with hardly any ſenſible effect. The loſs of water was not more than 0.2 of an ounce meaſure.

After uſing *charcoal* in the experiments recited in the preceding ſection, I went through one proceſs with *coak*, or the cinder of pit coal, and found that from 174 grains of coak, and with the loſs of 528 grains of water, I got 1700 ounce meaſures of air, of which one fifth was fixed air, and 30 ounce meaſures of it weighed ten grains leſs than an equal bulk of common air. The analyſis of this air will be found in the ſection appropriated to that ſubject.

On *iron ore* this proceſs had no effect. The ſame was the caſe with the trial I made of *quick lime*, and ſuch would probably be the caſe with dephlogiſticated earths in general.

With *manganeſe*, however, the reſult was different. Having made the ſteam of four ounces of water paſs over 828 grains of this ſubſtance, which had been expoſed to a ſtrong heat in an earthen retort ſome time before, I got 35 ounce meaſures of air, almoſt the whole of which was pure fixed air, with a reſiduum a little better than common air. The manganeſe had loſt 132 grains, and from being *black*, was become very *brown*. Again, I tranſmitted the ſteam of eight ounces of water over two ounces and an half of manganeſe, and got about 100 ounce meaſures of pure fixed air, with a reſiduum a little phlogiſticated. The manganeſe had loſt 112 grains.

SECTION XXIV.

Experiments relating to Iron.

THE moſt important of all the *metals* is unqueſtionably *iron*, and it ſeems to be that which is as yet the moſt imperfectly underſtood, and in the manufacture of which there is the greateſt room for farther improvements. But as all experimenters in air make uſe of it, at leaſt for the purpoſe of procuring inflammable air (that being the cheapeſt proceſs by which they can make it) it falls daily under their obſervation, and in conſequence of this it may be hoped, that we ſhall gradually gain a more perfect knowledge of it, and that this knowledge will lead to improvements in practice. I cannot promiſe much with reſpect to my own obſervations on the ſubject, but all *new facts* ought to be reported, and ſome that have occurred to me are of ſuch a nature,

as,

as, in the hands of manufacturers in a large way, may perhaps be of ſome utility.

One of the greateſt differences with reſpect to iron is between that which is called *caſt iron* (which is the ſtate in which it comes from the furnace) and that which is *malleable.* In the former ſtate, though exceedingly hard, it is brittle; ſo that it can no more be brought into any other form without melting, than glaſs, but when melted, it ſtill continues iron. In the malleable ſtate, it is no longer capable of being melted again, and continuing to be iron, but it is flexible, and without melting, may be hammered into any ſhape that is wanted.

I had formed an idea, from what I had formerly read on the ſubject, that this great change in iron was made by nothing more than violent *hammering* while it was red hot; but when I ſaw the real proceſs, I was ſoon convinced that this was a miſtake. For in the finery furnace (or that furnace which is intended to refine the iron, and make it malleable) the caſt iron is expoſed to a ſtrong blaſt of air, while it is red hot, or rather in a half melted ſtate; and it is this blaſt of *air* that is the moſt neceſſary, if not the only neceſſary circumſtance, in producing this change. Alſo, while the iron undergoes this operation, a quantity of liquid matter runs from it, and when it is cold acquires the form of a black cinder; which, from the name of the furnace out of which it flows, is called *finery cinder.* The nature of this ſubſtance, I think, I have inveſtigated. When this finery cinder is ſeparated from the common maſs, what remains of the iron is actually malleable; but being in looſe ſpungy maſſes requires the hammer to conſolidate it, and to reduce it into a proper form for farther operations. Alſo, a conſiderable quantity of finery cinder remains in the interſtices of the ſpungy maſſes, and the hammer is uſeful in ſeparating it.

A farther remarkable change in the conſtitution of iron, is that by which it is converted into *ſteel,* which is made by *cementing* it with charcoal. It is then capable of being made elaſtic, and of receiving various degrees of hardneſs, according to the ſuddenneſs with which it is cooled after it has been made red hot. It is alſo then capable of being melted, without loſing its metallic character.

At Birmingham, I find it is common to ſubject caſt iron to this proceſs of cementation; and then it is ſaid to be *annealed.* This gives it a ſlight degree of flexibility, ſo that it may be uſed for ſome kinds of nails. Having an opportunity of procuring theſe nails, both before and after annealing, I have made many experiments on them, and find that this proceſs makes a remarkable difference in the chymical properties of the metal; and ſuch as I ſhould not have expected *a priori.*

Iron that is annealed, as well as that which is malleable, and alſo ſteel, is readily diſſolved in diluted oil of vitriol, and leaves very little reſiduum; whereas the caſt iron that has not been annealed, diſſolves with great difficulty, and when it is diſſolved, leaves a great quantity of black reſiduum. The ſolution itſelf is alſo attended with ſome appearances of a very ſtriking nature, which I ſhall deſcribe as they occurred to me.

From twenty-four grains of caſt iron, in the form of nails, I got, by oil of vitriol diluted with about three times as much water, in the courſe of ſome weeks (for without external heat leſs time is not ſufficient) 69 ounce meaſures of inflammable air, exceedingly offenſive to the ſmell. The nails ſtill retained their form, and as after repeatedly heating the mixture till it boiled, very little more air came from them, I examined them, and found them to be pretty ſoft, ſo that I could eaſily cut them with a knife. They had alſo the ſame highly offenſive ſmell with the air that had come from them; and being put into a jar of common air, they ſoon diminiſhed, or phlogiſticated, it.

When theſe nails were dry, they were brittle; and though white on the outſide, they were black within; and after being haſtily dried, they appeared to have gained fifteen grains. This experiment explains what I am told is commonly obſerved by thoſe who put iron pipes into pits, in which there is water impregnated with vitriolic acid. For in time they become quite ſoft, or, as they call it, *rotten*, ſo that they can be cut with a knife. That a good deal of all the conſtituent parts of iron remains in this ſubſtance, was evident from the reſult of my melting a piece of it with a burning lens. For though it neither gained nor loſt any weight, it ſpirted a little in the fuſion, and threw out ſparks, juſt like caſt iron in the ſame experiment.

I was too haſty, however, in concluding that the proceſs above mentioned was over, as the next experiment will ſhew. Half an ounce of caſt nails had been kept in oil of vitriol, diluted with three times as much water, about three weeks, and though the menſtruum had been changed ſeveral times, and they had yielded 130 ounce meaſures of air, they retained their form. But I then perceived ſome ſpecks of black matter floating in the liquor, and after a few days more, I found one-third of the phial in which the ſolution was made quite opake with them. Nothing then remained in the form of a *nail*; and during this breaking up of the nails, the air had come from them much more copiouſly than it had done for a conſiderable time before. For the whole quantity of air was now 190 ounce meaſures.

The air, however, comes pretty readily at the beginning of this proceſs. For 60 ounce meaſures were procured in a very few days, whereas that quantity was not got in as many weeks afterwards, till the form of the nails was deſtroyed. The black powder that remained after this proceſs had a very offenſive ſmell; and when dried haſtily, weighed 18 grains. From another equal quantity of theſe nails this black powdery reſiduum weighed ſeventeen grains and a half, and this was the uſual proportion of its weight to that of the iron.

What this *black matter* really is, how the proceſs of annealing can ſo greatly leſſen the quantity of it, and what connexion this circumſtance has with the ſolubility of the iron in acids, are queſtions that well deſerve

ſerve conſideration; and I think it very poſſible, that a little attention to the ſubject may lead to an eaſy ſolution of them, and a farther inſight into the nature of iron.

Another phenomenon in the ſolution of this caſt iron being exceedingly remarkable, and with reſpect to myſelf, an *unique* (for I have never been able to repeat the obſervation) I ſhall mention the particulars exactly as they occurred to me, and as I noted them in my regiſter at the time.

In the courſe of laſt winter, I had put half an ounce of caſt iron nails into a mixture of oil of vitriol and water, and in the ſpace of about two months, in the cold weather, they had yielded 72 ounce meaſures of air; but the menſtruum not having been changed, much copperas was concreted among the nails. In the following ſpring, when I began to give more attention to the proceſs, I changed this liquor, and occaſionally applied to it the heat of a candle, in order to accelerate the ſolution. A few days after this I obſerved in the liquor a great number of *tranſparent filaments,*

ments, a quarter, or half an inch in length, like the fineſt hairs, part of them adhering to the nails, at the ends, but the greateſt quantity of theſe *hair-like cryſtals,* as they may be called, were detached from them, and floated in the liquor.

Taking ſome of them out of the menſtruum, I found that they diſſolved in freſh water, but not immediately. I then took them all out of the phial, and to the ſame nails put freſh oil of vitriol and water; and in a few days more of theſe cryſtals appeared, but not ſo many as before. On heating the liquor while they were in it, they all diſappeared; but after a few days they appeared again, and kept increaſing very much. I made them diſappear a ſecond time in the ſame manner, and with the return of cold they appeared again. Having poured off this liquor, with the cryſtals in it, I heated that liquor, and made them diſappear, but they re-appeared in the ſame liquor when it was cold, and no more in the freſh liquor that was put to the nails. After this the nails broke up, and the air came very rapidly, as in the former proceſs, and, in all, theſe nails yielded 163 ounce meaſures of air.

As theſe cryſtals were formed a little time before the nails broke up, I imagined that, whatever the ſubſtance was, it was that which had held them together, preventing their eaſy ſolution, and that it was attracted from the iron in the operation of annealing. I alſo thought, that by repeating this proceſs, and watching it a few days before the breaking up of the nails, I could not fail to find the ſame appearance. But though I repeated the experiment, and with all the variations that I could think of, I was never gratified with a ſecond ſight of theſe beautiful cryſtals.

As theſe cryſtals were evidently of a ſaline nature, I concluded they were only martial vitriol, though in an unuſual form. To aſcertain this, I diſſolved all the fragments I could collect of theſe cryſtals in water, and ſet it to evaporate. But inſtead of finding any green vitriol, there was only a black incruſtation left on the evaporating veſſel.

There has been ſome difference of opinion among chymiſts concerning the proportional quantity of air yielded by *iron* and *ſteel*. Having taken ſome pains to aſcertain theſe facts, at the ſame time that I was diſſolving iron and ſteel for other purpoſes, I ſhall here mention the reſult of my obſervations reſpecting the air that is yielded by all the kinds of iron that have fallen under my notice. I repeated the experiments ſeveral times, but the reſults often varied, without my being ſenſible of any change in the circumſtances of the experiments.

What quantity of air came from *caſt iron* in the experiments mentioned above, I have noted in the account of them; but as I frequently repeated the experiment, eſpecially with a view to find the hair like cryſtals that I have mentioned, I ſhall here put down the different quantities of air, of which I alſo took an account in ſome of them, as well as of the reſiduums of black matter, which I obſerved at the ſame time. At one time a quarter of an ounce of caſt iron gave ninety ounce meaſures of air, half an ounce gave 163 ounce meaſures, and two ounces gave 727 ounce meaſures, leaving a reſiduum of black powder, which weighed ninety-eight grains; and four ounces of the iron gave a reſiduum of 163 grains. Upon the whole, therefore, a quarter of an ounce of this iron may be ſaid to give ninety ounce meaſures of inflammable air.

Of the *annealed caſt nails*, a quarter of an ounce gave at one time 101 ounce meaſures of air, at another time 105½, at another 106, and the moſt of all 107. Half an ounce gave 212 ounce meaſures. Half an ounce of iron turnings, which is iron leſs perfectly annealed, gave ninety-ſix ounce meaſures of air. It is evident, therefore, that the annealed caſt iron gives conſiderably more inflammable air than that which had not been annealed; and this is eaſily accounted for, from the much greater quantity of reſiduum from the iron that has not been annealed, which reſiduum, it will be ſeen, contains much phlogiſton.

A quarter of an ounce of *ſteel* gave ninety ounce meaſures and an half of air, 118½ grains gave ninety-five ounce meaſures and an half of air, and the ſame weight of the very ſame iron from which that ſteel had been made gave ninety-ſix ounce meaſures. Half an ounce of ſteel gave 207 ounce meaſures, and the black reſiduum, though greater than is found in the ſolution of iron, or caſt iron annealed, was too ſmall to be weighed. At other times, however, it has been much more conſiderable, though I have no note of the exact quantity that I found in any experiment. It is alſo obſervable, that in the ſolution of this half ounce of ſteel, in which there was ſo little reſiduum, the quantity of air was unuſually great. And, I preſume, that the differences in the quantities of air from given weights, both of ſteel, and different kinds of iron, will always be in ſome reciprocal proportion to the quantity of this reſiduum, which I have never found any ſolution of iron to be entirely without; and this will account for the different quantities of air that different perſons have reported to have found both in iron and in ſteel. As a greater quantity of inflammable air was procured from annealed caſt iron, than from malleable iron, I think it may be concluded, that ſteel (which is malleable iron annealed) will always give more air than iron, provided that, in the ſolution, it ſhould yield no greater a quantity of black reſiduum. But in general there is much more of it found in the ſolution of ſteel than in that of iron.

This black powder which remains from the ſolution of iron or ſteel, and eſpecially that from the caſt iron not annealed, which is in ſuch great quantity, well deſerves to be thoroughly examined. Having made a few miſcellaneous experiments upon it, I ſhall here give the reſult of them. Putting eighteen grains of it into ſpirit of ſalt, there remained eight grains undiſſolved. This was of a grey colour, but being ſprinkled upon hot iron, it did not appear, by this teſt, to contain any *ſulphur*.

I expoſed a quantity of it to the heat of the burning lens in vacuo, and found the air that was expelled from it to be one ninth fixed air, and the reſt inflammable, of the exploſive kind. Ten grains of the powder gave nine ounce meaſures of air; but a good deal of this light powdery ſubſtance was unavoidably loſt in conveying it through the water after the proceſs.

This matter alſo appears to contain ſomething of iron, though it is no longer ſoluble in oil of vitriol. For being expoſed to the heat of the lens in open air, it was melted, and weighed juſt the ſame before and after fuſion, which had alſo been the caſe with that caſt iron which had been imperfectly diſſolved in oil of vitriol, and retained its form.

From the air which this black powder gave, it may be concluded, that it contains much *plumbago*; but that which remains after the ſolution of malleable iron, or ſteel, is more nearly, or perhaps perfectly ſo. Nineteen grains of this (though it is poſſible that ſome of the other might have been accidentally mixed with it) was reduced to ſix grains, by being melted with the heat of the lens in the open air, and then became a glaſs, or ſlag; which was nearly the ſame reſult that I had from genuine plumbago, the greateſt part of which is reduced into fixed and inflammable air.

The proceſs of *cementation* is not, I am perſuaded, ſufficiently underſtood. It is the opinion of thoſe who make ſteel, that the metal neither gains nor loſes weight in the proceſs. Thoſe who anneal caſt iron tell me, that it loſes conſiderably in that proceſs, which is ſimilar to that of making ſteel, though the iron is taken in very different ſtates. On the contrary, I found both malleable iron and caſt iron to gain a little weight by cementation in my fires. Seventy-two grains of iron wire gained three grains, and became of a dark black colour, and three ounces of caſt iron nails gained ſix grains. But perhaps the heat that I uſed was too great for the purpoſe. For in conſequence of this iron will attract water, and become in part *ſcales of iron,* which is always attended with an increaſe of weight; whereas in the ſame proceſs with a long continued and moderate heat, it is very poſſible that ſome of the elements of that black matter, or plumbago, may go out of the iron, and join the charcoal, forming perhaps a ſulphur, which may be ſublimed, and diſperſed in the proceſs.

I have obſerved that I once expelled from ſome annealed caſt iron nails ſuch inflammable air as conſtituted fixed air, when it was decompoſed with dephlogiſticated air, which ſhews that it had imbibed ſome of that kind of inflammable air which is peculiar to charcoal. But when I diſſolved that kind of iron in vitriolic acid, the air was the very ſame with that which came from malleable iron, no fixed air being produced in the decompoſition of it.

SECTION XXV.

Experiments on Finery Cinder.

THE moſt uſeful of my obſervations on the ſubject of *iron* relates to the nature of the *finery cinder*, the ſubſtance which, I have obſerved, runs in a liquid form from caſt iron in the proceſs of converting it into malleable iron. This has generally been conſidered as a thing of no value; and is commonly thrown away as ſuch, though my brother-in-law, Mr. WILKINSON, has, with advantage, made uſe of a certain proportion of it in the ſmelting of iron. I flatter myſelf, however, that by a due attention to ſome obſervations which I ſhall here relate, this ſubſtance may hereafter be employed to more advantage. But before I mention this uſe of it, in reducing it to iron, I ſhall recite ſome experiments which I made with a view to aſcertain the nature of it.

The firſt thing that occurred to me, was to attempt the ſolution of it in the ſeveral mineral acids, which I did with the following reſults. Putting 60 grains of it, finely pulverized, into each of the three acids, and leaving them two or three days to digeſt; of that which had been in the vitriolic, and alſo of that which had been in the marine acid, there remained 15 grains undiſſolved; but of that which had been expoſed to the nitrous acid, 40 grains. Another time, I obſerved that of 74 grains expoſed to the ſpirit of ſalt, 30 remained undiſſolved.

This ſubſtance, as it comes from the common furnace, is ſeldom without ſome portion of iron, as appeared by melting it in the ſun, when 24 grains of it gained one grain in weight. It alſo gained a little weight when I expoſed it to the action of ſteam in an hot earthen tube.

Finery cinder, when hot, readily unites with moſt kinds of earth, greatly promoting their fuſion, in the ſame manner as the *fluor*, which has its name from that circumſtance. Attempting to get air from the finery cinder in an earthen retort, 70 ounces of it yielded 40 ounce meaſures of air; but as the retort was diſſolved, and moſt of the contents run out, nothing certain could be pronounced concerning the origin of this air, of the laſt portion of which three-fourths were fixed air. The ſtandard of the reſiduum was 1.67. In one part of it a candle was extinguiſhed, but another portion was ſlightly inflammable.

The circumſtance that firſt gave me an inſight into the real nature of this finery cinder, was my having found a piece of malleable iron become throughout a perfect finery cinder, in conſequence of being left in the open fire of my furnace, when the heat had been very great. The appearance of it alone ſatisfied me, that it was the ſame thing; and comparing it in a variety of proceſſes with real finery cinder, I found that they did not differ in any reſpect. They appeared alſo to be the ſame ſubſtance with the *ſcales* which fall off from iron when it is heated in an open fire, with that which malleable iron is brought to by being melted in the open air, or in dephlogiſticated air, and alſo by being expoſed to the action of ſteam when it is red hot. In all the different proceſſes recited in my paper on phlogiſton, it was perfectly indifferent which of theſe ſubſtances I made uſe of, and therefore, I omit to mention the particulars.

Finery cinder is therefore to be conſidered as iron increaſed about one third in weight by imbibing pure air, or rather water, though with the loſs of its own phlogiſton. Nothing, therefore, is wanting to bring it to the ſtate of iron again, but the expulſion of the pure air or water, with which it is ſaturated, and giving it the phlogiſton which it has loſt: and this is readily done by heating it in cloſe veſſels, in contact with any thing that contains phlogiſton. This is effected in the moſt complete manner, by heating it with a burning lens in inflammable air. But it is likewiſe done by cementation with charcoal, with coak from pitcoal, or with raw coal. In all theſe proceſſes, the finery cinder loſes about one third of its

weight,

weight, and is then perfectly ſoluble in acids, and attracted by the magnet. Conſequently, it is *perfect iron*. But whether it can be made uſeful iron, and in a manner ſo cheap as to make it worth the while of the manufacturer of iron, to eſtabliſh any work for the purpoſe, is not for me to ſay. I would only obſerve, that iron thus made from finery cinder in my retorts is not in the ſtate of ſimple *caſt iron*, but of that which is annealed, and likewiſe partially malleable.

SECTION XXVI.

Of Air acting through a Bladder.

ONE of my former experiments which I was leaſt able to account for, was the diminution of nitrous air in a bladder ſwimming at liberty in a trough of water; the conſequence of which had always been, that in a few days the nitrous air was diminiſhed to about one fourth, and this was phlogiſticated air. All the progreſs that I had then made in the inveſtigation of this curious fact, was finding that it depended, as I then thought, upon the bladder being kept alternately dry and moiſt; becauſe when the bladder was kept covered with water, it remained full, and the air within it was not changed. This was alſo the caſe when the bladder was kept dry. But I did not conſider that when the bladder was kept under water, there was no *air* in contact with it; and I did not then ſuſpect that this change in the air depended on the action of the nitrous air upon the external common air through a moiſt bladder; though I had found that coagulated blood has a power of acting upon air, and is of courſe liable to be acted upon by air, through any bladder.

At length, ſuſpecting that this *might* be the caſe, I made the following experiment. Taking a bladder which contained twenty ounce meaſures of nitrous air, and tying it very tight, I introduced it into a glaſs jar, which contained forty ounce meaſures of common air; becauſe, in that proportion, they would be able, if they had any mutual action, to ſaturate one another. Wiſhing at the ſame time, to obſerve the changes that might gradually take place in each of the kinds of air, I examined them both at different periods.

The proceſs was begun on the 18th of May, and on the 21ſt I found that there were only 34 ounce meaſures of the common air, and eleven of the nitrous, the bladder being quite ſound; ſo that it was ſufficiently evident, that the two kinds of air, had affected each other through the ſubſtance of the bladder. On the 25th of the ſame month there were 31½ ounce meaſures of the common air, and four and a half of the nitrous; and examining the ſtate of both of them, I found the ſtandard of the common air to be 1.8, which was a ſtate very near that of extreme phlogiſtication; and that of the nitrous 1.7. That is, equal meaſures of this and of common air, occupied the ſpace of 1.7 meaſures, which ſhews that it had almoſt loſt its power of affecting common air, or to expreſs myſelf perhaps more correctly, there was but a ſmall proportion of nitrous air in it.

On the 8th of June I examined them for the laſt time, after having obſerved no farther change for ſome days in the quantity of the common air (as indicated by marks which I had made on the outſide of the jar) and I found only 28 ounce meaſures of the common air, of the ſame quality as when I had examined it before, viz. of the ſtandard of 1.8, and only three ounce meaſures of the nitrous air, which did not affect common air at all. Neither of them contained any portion of fixed air, and both of them extinguiſhed a candle.

Nothing now remained to my complete ſatisfaction, with reſpect to my former obſervation of the diminution of nitrous air, contained in a bladder. But I farther wiſhed to ſatisfy myſelf with reſpect to the action of *inflammable air*, on either common or dephlogiſticated air, in the ſame circumſtances. Nitrous air affects pure air by ſimple contact, without ignition; whereas, inflammable air, I had obſerved, has very little effect upon pure air when they are ſimply mixed together. I was, therefore, ſurprized to find that inflammable air has a very conſiderable action upon dephlogiſticated air through a bladder, without any aſſiſtance from heat; and moreover, that the union of theſe two kinds of air, thus produced, forms fixed air. The experiments which I made for this purpoſe, were as follows.

Into a jar containing 123 ounce meaſures of dephlogiſticated air, I introduced a bladder, containing 23 ounce meaſures of inflammable air; and after a few days, I obſerved that the bladder in which it was contained was become a little flaccid. After about three weeks, I examined both the kinds of air, and found that the bladder contained only two ounce meaſures, and that this was no longer inflammable, but extinguiſhed a candle, though it had in it a mixture of pure air. The air within the jar then contained one twentieth of its bulk of fixed air. The dephlogiſticated air was diminiſhed ſeven ounce meaſures; and from being of the ſtandard of 0.5, with two equal meaſures of nitrous air, it was now become of 1.4. The bladder had a ſlight ſmell of putrefaction, but it was perfectly air tight.

It is obſervable, that in this experiment part of the dephlogiſticated air had paſſed unchanged into the bladder of inflammable air, whereas the inflammable air which had paſſed through the bladder into the dephlogiſticated air, had united to it, and formed fixed air. The tranſmiſſion of the dephlogiſticated air through the bladder was much more remarkable in the following experiment.

Having introduced a bladder filled with inflammable air into a large jar of dephlogiſticated air, the bladder, after two days only, had in it a great mixture of dephlogiſticated air, and was as much diſtended as when it was firſt put into the jar. A quantity of it exploded exactly like a mixture of one-third dephlogiſticated, and two-thirds inflammable air. The bladder was perfectly ſound and ſweet, and the dephlogiſticated air was not ſenſibly altered.

Again, having introduced a bladder containing ten ounce meaſures of inflammable air into a jar containing 100 ounce meaſures of dephlogiſticated air, of the ſtandard of 0.3, I found, about a month afterwards, that the air in the jar was diminiſhed to 90 ounce meaſures, and the inflammable air to five ounce meaſures and an half. The quality of the air in the bladder and of that in the jar was very nearly the ſame, though the bladder was perfectly ſound and ſweet. The air in the bladder, with equal meaſures of nitrous air, was of the ſtandard of 0.76, and that in the jar of 0.74. Both of them alſo contained a ſmall portion of fixed air. In this caſe, therefore, both the kinds of air had not only been tranſmitted through the bladder, but ſome decompoſition had alſo taken place within it, as well as within the jar.

In another experiment of this kind, both the bladder of inflammable air, and the jar of dephlogiſticated air, after ſome time, contained each of them a portion of fixed air, and likewiſe both the kinds of air unaffected by each other. For both of them exploded when they were examined ſeparately.

I had ſome ſuſpicion that the fixed air in this proceſs might have come from the putrefaction of the bladder; eſpecially as in one of the caſes the ſmell of it was very ſenſibly putrid. To determine this, I put a bladder filled with inflammable air into a jar containing inflammable air likewiſe, and let it continue there till it was quite putrid, and diſſolved. But after this I found no ſenſible quantity of fixed air in the jar. I therefore concluded, that the fixed air which I found in the preceding experiments, came undoubtedly from the union of the inflammable and dephlogiſticated air, in conſequence of their acting upon each other through the ſubſtance of the bladder.

It ſeems to follow from theſe experiments, that fixed air is really *formed* when inflammable air of charcoal, &c. is exploded together with dephlogiſticated air; and alſo that the greatneſs of the heat prevents its formation, when inflammable air from metals is uſed. For though, in the exploſions with the electric ſpark, no fixed air was produced from the decompoſition of the pureſt inflammable air, it was evidently ſo with the ſame kind of inflammable air in theſe experiments with a bladder, in which no heat is uſed.

SECTION XXVII.

MISCELLANEOUS EXPERIMENTS.

1. *Of the Influence of Water on Phlogiſticated Air.*

IN the infancy of my experiments I concluded that all kinds of air were brought by agitation in water to the ſame ſtate, the pureſt air being partially phlogiſticated, and air completely phlogiſticated being thereby made purer; inflammable air alſo loſing its inflammability, and all of them being brought into a ſuch a ſtate, as that a candle would juſt go out in them. This inference I made from all the kinds of air with which I was then acquainted, and which did not require to be confined by mercury, being brought to that ſtate by agitation in a trough of water, the ſurface of which was expoſed to the open air; never imagining that when the air in my jar was ſeparated from the common air by a body of water, generally about twelve inches in depth, (adding that within to that without the jar) they could have any influence on each other. I have, however, been long convinced that, improbable as it then appeared to me, this is actually the caſe, though a fact ſo remarkable well deſerves to be farther attended to.

As I have no where recited any experiments which prove that this change in the ſtate of the air, agitated in water, depends upon that water having ſome communication with the atmoſphere, I ſhall do it here, after an account of ſome other experiments, from which it is equally evident that, notwithſtanding every poſſible precaution, the air will be changed if there be that communication.

I agitated, a quarter of an hour, about three ounce meaſures of air, phlogiſticated with nitrous air, in a veſſel containing 20 ounces of water, which had been boiled ſeveral hours, and while it was very warm;

after which it was diminiſhed one-ſixth, and was ſo much improved, that the ſtandard of it was 1.74. The next day I agitated the remainder of it another quarter of an hour in the water which had been boiled at the ſame time, when it was diminiſhed about one-tenth, and the ſtandard of it was 1.57.

I alſo agitated the ſame quantity of air, phlogiſticated with iron filings and brimſtone, in the ſame manner twenty minutes, when it was diminiſhed one-ſeventh, and the ſtandard of it was 1.66.

I afterwards expelled a little air from the water in which this air had been agitated; and from that with which the firſt of theſe experiments was made, I got air, the ſtandard of which was 1.66; and from the water in which the ſecond experiment was made air of the ſtandard of 1.33. In this a candle would have burned. It is evident, from the diminution of the air in theſe experiments, that part of it is abſorbed by the water. But it is no leſs evident, as will appear by the next experiment, that the place of ſome part of the air which had been abſorbed was ſupplied by a purer air, which the water had imbibed from the atmoſphere. That water does imbibe the purer part of atmoſpherical air, in preference to that which is impure, is evident from any examination of it. For if the water be clear, and free from any thing that is putreſcent, the air expelled from it by heat is generally of the ſtandard of 1.0; whereas that of the atmoſphere, when the nitrous air is the pureſt, is about 1.2.

But the experiment which deciſively proved that my former concluſion was fallacious was the following. After expelling all air, by boiling, from a quantity of water, I put to it, in ſeparate phials, air that had been phlogiſticated with iron filings and brimſtone; and corking them very well, I left them with their mouths in water, and I occaſionally agitated them. After about two months I examined them, and I found both the air that was confined by the water, and alſo that air which by heat I expelled from the water, completely phlogiſticated.

giſticated. The ſame was the reſult of a ſimilar experiment, in which the air had been expelled from the water by the air pump.

2. *Of Air produced more or leſs rapidly.*

IT was one of my firſt obſervations on the ſubject of air, that, in ſeveral caſes, a much greater quantity of air was produced by a rapid, than by a ſlow proceſs. I have ſeveral times verified this fact, though I cannot very ſatisfactorily account for it. In my late experiments I had an inſtance of it, which it may be proper to recite.

From an ounce and an half of whiting in a gun barrel I got, with a ſudden and great heat, 240 ounce meaſures of air, of which ſome portions contained nine tenths of fixed air, and the reſiduum burned with a lambent blue flame. Repeating the ſame proceſs very ſlowly, I got no more than 150 ounce meaſures of air from the ſame quantity of whiting. And again repeating the proceſs with a greater and a more ſudden heat than at firſt, I got, from the ſame quantity of materials, 270 ounce meaſures of air.

3. *Of different kinds of Air remaining diffuſed through each other without uniting.*

IN my former publications I obſerved that, when any two kinds of air, of the greateſt different ſpecific gravities, and without any known cauſe of attraction to each other, are mixed together in the ſame veſſel, they are never ſeparated by mere reſt, but continue equally diffuſed through each other. This at leaſt I found to be the caſe with reſpect to all the kinds of air that can be confined by water, and ſeveral of thoſe that only can be confined by mercury, except that *inflammable* air always remains *ſeparate* from acid or alkaline air, though contained in the ſame veſſel. I had lately a very clear proof of this with reſpect to dephlogiſticated and inflammable air.

Having filled the ſtrong glaſs veſſel, fig. 5, in which I uſually fire them by the electric ſpark, with the uſual mixture of one third dephlogiſticated and two thirds inflammable air, I let it remain in a perpendicular poſition all night, in which there was certainly time enough for them to have ſeparated from each other, if their very different ſpecific gravities would have diſpoſed them to do ſo. But though the two wires between which the exploſion is made were at the very top of the veſſel, which in the caſe of a ſeparation would have been the region of pure inflammable air (which cannot take fire of itſelf) yet when the ſpark paſſed between them, the two kinds of air were exploded as completely as they could have been immediately after they had been mixed.

It is alſo obſervable, that the phlogiſticated and dephlogiſticated air which compoſe the atmoſphere are of very different natures, though without any known principle of attraction between them, and alſo of different ſpecific gravities; and yet they are never ſeparated but by the chemical attraction of ſubſtances, which unite with the one, and leave the other, as in all thoſe proceſſes which I have termed phlogiſtic.

4. *Of the Ice of Water impregnated with Vitriolic Acid Air.*

IT is remarkable that water impregnated with vitriolic acid air retains all its air when it is frozen, though every other kind of air (if the liquor containing it can be frozen at all) is ſeparated from it in the act of freezing. I have now farther obſerved, that this ice ſinks in the liquor from which it is frozen, in which it reſembles the ice of oil. This is a fact which I barely mention, without having any theory to account for it.

5. *Of the Precipitate from the Solution of Metals by Heat.*

IN my fourth volume of experiments I have recited many on liquors expoſed to a long continued heat, in glaſs tubes hermetically ſealed, and among other things obſerved a remarkable depoſit from the ſolution of *mercury*, and alſo that of *copper* in ſpirit of nitre. But they both require a conſiderable *time*. I have ſince found that the depoſit from *iron*, and from copper in volatile alkali, are made much ſooner.

In a glaſs veſſel hermetically ſealed, a good deal of iron was precipitated in the form of *red earth* from a weak ſolution of it in ſpirit of ſalt, placed in a ſand heat, only a ſingle day. It was alſo precipitated in great abundance from a ſolution of ſpirit of nitre in the ſame time. In this caſe the veſſel was not quite cloſed, ſo that a little could evaporate. The remaining liquor was quite colourleſs.

Copper was precipitated in the form of a deep blue earth, from a ſolution of it in volatile alkali at the ſame time; but the remaining liquor was almoſt as blue as at firſt. The ſolid precipitate ſmelled ſtrong of the volatile alkali, as well as the ſolution itſelf.

6. *Of heating Iron in fluor acid Air.*

HAVING mentioned the effect of heating various ſubſtances in different kinds of air by means of a burning lens, that I may not omit to mention any thing that I have obſerved of the kind, and that I can think any perſon might wiſh to be informed of, I ſhall, in this miſcellaneous ſection, recite the following experiments.

Throwing the focus of the lens upon iron turnings in fluor acid air, a denſe white fume preſently filled the veſſel, and during the proceſs the heat was remarkably great, ſo that I could not touch the upper part of the glaſs veſſel in which the experiment was made. The quantity of air was conſiderably diminiſhed, and a quantity remained that was not abſorbed by water, but not enough to examine it.

7. *Of the Solution of Precipitate per ſe in Spirit of Salt.*

PRECIPITATE *per ſe* diſſolves with great rapidity in ſpirit of ſalt. A quantity of this ſolution I impregnated with nitrous vapour, on which the ſurface of the fluid, and the ſides of the phial, were inſtantly covered with cryſtals, larger than thoſe of the precipitate. The colour of the liquor was then of a light blue, or green. Afterwards it aſſumed a deep brown, inclining to yellow, and a great quantity of white matter was depoſited, occupying almoſt the whole ſpace of the liquor. This muſt, I preſume, have been corroſive ſublimate, the pure air from the precipitate having dephlogiſticated the ſpirit of ſalt, which is a neceſſary circumſtance in this preparation.

8. *Of Air extracted from the Water that is uſeful in fertilizing Meadows.*

IT has been found that meadows watered with a freſh ſpring produce abundance of graſs without any manure. Having had reaſon to think that phlogiſton, adminiſtered in ſome form or other, is the food of plants, I imagined that this water, being in itſelf pure, and free from all putreſcent matter, muſt contain phlogiſticated air, eſpecially as I had been told that the water is only uſeful after its firſt iſſuing from the earth, and that the firſt meadows it overflows are thoſe which receive the moſt benefit from it.

I therefore deſired Mr. YOUNG, to whom this country owes ſo much for the attention that he has given to the buſineſs of agriculture, to ſend me a quantity of ſuch water as had been found to be moſt uſeful in fertilizing meadows. He was ſo obliging as to ſend me two bottles of ſuch water; and as ſoon as I received them I expelled from them the air they contained, and found that which was in one of them, ſufficiently impure, the ſtandard of it being 1.4; but that in the other, was ſuch as is uſually contained in water that has been long expoſed to the atmoſphere, being of the ſtandard of 1.0; ſo that I imagined it had not been ſufficiently well corked. After the water in the other bottle had been expoſed a few days to the open air, the air contained in it was of the ſame quality. Upon the whole, I think that the experiment was ſufficiently favourable to the hypotheſis, but more ought to be made, and if poſſible, on the ſpot.

9. *Of*

9. *Of Water heated in Gun Barrels welded up.*

AS inflammable air was produced in ſome experiments, in which I endeavoured to change the nature of water by making it red hot in a gun-barrel, the orifice of which was welded up, it may not be improper juſt to mention them in this place. They will likewiſe ſerve to ſhew the expanſive force of water in that ſtate. The experiments were made in March 1783.

Putting 16 grains of water into a gun-barrel, containing 4½ ounce meaſures, I got it welded up; and making it red hot, it burſt in the middle after a few minutes. I aſcertained the quantity of water, by putting it into a ſmall glaſs tube, which I ſealed hermetically, and put within the gun-barrel.

I then put ſix grains of water into the thicker half of a muſket barrel, and three grains and a half into a thinner barrel. Theſe did not burſt when they were red hot, and being pierced under water, inflammable air ruſhed out. I repeated theſe experiments, and always had the ſame reſult; inflammable air being procured, when the gun-barrels were opened under water; and if the water was in ſufficient quantity, part of it at leaſt (for I could not meaſure it with exactneſs) was found in the barrel.

10. *Of the Solution of Iron in phlogiſticated and dephlogiſticated nitrous Acid.*

THAT the giving of nitrous air depends upon phlogiſton, is evident from the phenomena which attend the ſolution of iron in phlogiſticated and dephlogiſticated acids. Pouring a ſmall quantity of phlogiſticated nitrous acid into a large quantity of water, which had iron wire in it, it preſently became of a dark colour; but this was ſoon precipitated, and the ſolution aſſumed a lighter colour. I then poured off the ſolution, which was of a ſlight brown colour, and pouring into it more phlogiſticated nitrous acid, it immediately became of a very dark colour, and emitted air copiouſly.

On examination it appeared to be ſtrong nitrous air. After the emiſſion of this air, the dark colour diſappeared. Theſe phenomena, therefore, exactly reſembled thoſe of a ſolution of green vitriol, which aſſumes a dark colour by imbibing nitrous air, and becomes clear again by the expulſion of it. The dark green ſpirit of nitre had the ſame effect as the brown phlogiſticated acid, but the dephlogiſticated nitrous acid had no ſuch effect.

It is eaſy to make a pretty ſtrong ſolution of iron in dephlogiſticated nitrous acid that ſhall be green and give no air, if it be kept very cold during the proceſs. But if phlogiſticated nitrous acid be poured into the ſolution, it preſently becomes very black, and emits air. This blackneſs will ſome times, if the nitrous acid be very volatile, go off almoſt immediately; but in all caſes it will do ſo *in time*, and leave the liquor like water, or with a ſlight tinge of yellow; owing probably to part of the ochre having imbibed pure air, and thereby tending to become red.

Nitrous air alſo admitted to a green ſolution of iron in nitrous acid immediately turns it black, juſt as it does a ſolution of green vitriol.

Phlogiſticated nitrous acid dropped into a ſolution of green vitriol alſo makes it black. The green ſolution of iron in ſpirit of nitre, yields very little air by heat, and this is not nitrous air. When charcoal was put into it, and heated, it alſo gave little or no air.

SECTION XXVIII.

Obſervations relating to Theory.

IT is always our endeavour, after making experiments, to generalize the concluſions we draw from them, and by this means to form a *theory*, or *ſyſtem of principles*, to which all the facts may be reduced, and by means of which we may be able to foretel the re-

ſults

ſult of future experiments. With a view to this, it has of late been a great object with philoſophers to aſcertain the number of *elements* that are neceſſary to conſtitute all the ſubſtances with which we are acquainted, and eſpecially the different kinds of *air*, to which our attention has been much directed, in conſequence of their ſeeming to bring us a little nearer to the ultimate conſtituent parts of bodies; finding that by their union they are capable of forming ſolid maſſes.

In my former publications I have frequently promiſed to give ſuch a *general theory* of the experiments in which the different kinds of air are concerned, as the preſent ſtate of our knowledge of them will enable me to do. But, like Simonides with reſpect to the queſtion that was propoſed to him concerning God, I have deferred it from time to time; and indeed I am more than ever diſpoſed to defer it ſtill longer, as I own that I am at preſent even leſs able to give ſuch a theory as ſhall ſatisfy myſelf, than I was ſome years ago; new difficulties having ariſen, which unhinge former theories, and more experiments being neceſſary to eſtabliſh new ones.

Fluctuating, however, as the preſent ſtate of this branch of knowledge is, I do not think that I can, on this occaſion, intirely decline giving ſome obſervations of a theoretical nature; and though I cannot pretend to perform the whole of my promiſe, I ſhall, at the cloſe of this volume, give a ſummary view of what appears to me to be the conſtituent parts of all the kinds of air with which we are acquainted, and a more particular account of the hypotheſis concerning phlogiſton, which is at preſent more an object of diſcuſſion than any thing elſe of a theoretical nature. The ſketch that I ſhall give will at leaſt ſerve, like our former theories, to amuſe us when we look back upon it, after having gained a more perfect knowledge of the ſubject.

The only kind of air that is now thought to be properly *elementary*, and to conſiſt of a ſimple ſubſtance, is *dephlogiſticated air*, with the addition at leaſt of the principle of *heat*, concerning which we know very little;

and as it is not probable that this adds any thing to the *weight* of bodies, it can hardly be called an *element* in their compoſition. Dephlogiſticated air appears to be one of the elements of water, of fixed air, of all the acids, and of many other ſubſtances, which, till lately, have been thought to be ſimple. The air of the atmoſphere, excluſive of a great variety of foreign impregnations, appears to conſiſt of dephlogiſticated and phlogiſticated air.

Till Mr. CAVENDISH's fine experiment on *phlogiſticated air*, this was imagined to be a ſimple ſubſtance, and we entertained no hope of being able to decompoſe it; but in this, he has, to all appearance, happily ſucceeded, and, very contrary to my expectations, has ſhewn that it conſiſts of nitrous acid and phlogiſton; this acid having always been produced by decompoſing it with a ſufficient quantity of dephlogiſticated air. I ſee nothing to object to his experiment, and I conſider it as one of the greateſt diſcoveries on the ſubject of air; but I am at a loſs to account for ſome productions of phlogiſticated air, in which no nitrous acid is concerned. At leaſt we get air which has all the known properties of phlogiſticated air, as its extinguiſhing a candle, being fatal to animals, and not being affected either by nitrous air, or by lime water, in heating charcoal and red precipitate, in the decompoſition of alkaline air, &c. If the phlogiſticated air procured in theſe proceſſes be reſolvable into nitrous acid, like that of which the atmoſphere conſiſts, we ſhall be more embarraſſed than ever with reſpect to our theories concerning air, ſince the elements of nitrous acid muſt then be found where they were leaſt expected. Since, however, in my experiments, volatile alkali was produced from nitrous air and iron, the production of phlogiſticated air from alkaline air may not be thought extraordinary on this ſuppoſition, viz. of phlogiſticated air containing nitrous acid.

Perhaps ſome part of the difficulty may be relieved, by making a little alteration in the hypotheſis of Mr. CAVENDISH, and ſuppoſing, agreeably to the ſome experiments recited in this volume, that the *acid principle* is ſupplied by the dephlogiſticated air, while the nitrous air gives the baſe of the nitrous acid, and phlogiſton; and then this air may perhaps be conſidered as phlogiſton combined not with all the neceſſary elements of nitrous acid, but only what may be called the baſe of it, viz. the dephlogiſticated nitrous vapour, or ſomething which, when united to dephlogiſticated air, will conſtitute nitrous acid.

Fixed air ſeems to be a compound of phlogiſton and dephlogiſticated air, without any water, and for this reaſon, perhaps a red heat may be as requiſite to form the union between them, as it is to form that between phlogiſton and vitriolic acid, in the compoſition of ſulphur, which likewiſe contains no water. It is ſomething remarkable, that two ſubſtances, ſo different from each other as *fixed air* and *water*, ſhould be analyzed into the ſame principles. But there is this difference between them, that water is the union not of pure phlogiſton but of inflammable air and dephlogiſticated air, *Inflammable air* ſeems now to conſiſt of water and inflammable air, which, however, ſeems extraordinary, as the two ſubſtances are hereby made to involve each other, one of the conſtituent parts of water being inflammable air, and one of the conſtituent parts of inflammable air being water; and, therefore, if the experiments would favour it (but I do not ſee that they do ſo) it would be more natural to ſuppoſe that water, like fixed air, conſiſts of phlogiſton and dephlogiſticated air, in ſome different mode of combination.

There is an aſtoniſhing variety in the different kinds of inflammable air, the cauſe of which is very imperfectly known. The lighteſt, and therefore, probably, the pureſt kind, ſeems to conſiſt of phlogiſton and water only. But it is probable that *oil*, and that of different kinds, may be held in ſolution in ſeveral of them, and be the reaſon of their burning with a lambent flame, and alſo of their being ſo readily reſolved into fixed air, when they are decompoſed with dephlogiſticated air; though *why* this ſhould be the caſe I cannot imagine.

When inflammable air and dephlogiſticated air are burned, the weight of the water produced is never, I believe, found quite equal to that of both the kinds of air. May not the *light*, therefore, emitted from the flame, be part of the phlogiſton of the inflammable air, united to the principle of *heat?* And as light accompanies the *electric ſpark*, may not this alſo be the real *accenſion* of ſome phlogiſtic matter, though it is not eaſy to find the ſource of it?

Nitrous air, according to the long eſtabliſhed hypotheſis, conſiſts of nitrous acid and phlogiſton. But if I may be allowed to correct this theory, in the manner mentioned above, we muſt ſay that it conſiſts of a dephlogiſticated nitrous vapour, and phlogiſton; and that the addition of dephlogiſticated air, and alſo of water, is neceſſary to make it nitrous acid; ſuch at leaſt being no improbable inference from the experiments recited in this volume.

Dephlogiſticated nitrous air, I take to be dephlogiſticated nitrous vapour only, and therefore, perhaps, an elementary ſubſtance, as well as dephlogiſticated air. It ſeems to be produced by depriving nitrous air of its phlogiſton. But there are difficulties relating to this ſubject, which I ſhall mention preſently.

The different kinds of *acid air*, it ſeems to be agreed on all hands, conſiſt of the peculiar acids, in the form of vapour, and phlogiſton; ſo that vitriolic acid air conſiſts of the ſame principles with ſulphur, neither of them containing any water.

Fluor acid air, which I once thought to be nothing more than vitriolic acid air loaded with a peculiar ſpecies of earth, I now concur with others in thinking to conſiſt of a peculiar acid and that earth.

Alkaline air will appear, I think, from my late experiments, to conſiſt of ſome combination of inflammable air and phlogiſticated air, or of ſomething capable of being converted into phlogiſticated air; but the mode of combination, and the proportion of the ingredients, remain to be inveſtigated. That water enters into the compoſition of alkaline air, ſeems neceſſary to be admitted, becauſe it is decompoſed into inflammable air, which I cannot help thinking neceſſarily requires water. It ſeems, however, clearly to be inferred from my late experiments, that there is no occaſion to admit the *alkaline principle* into the number of *elements*; the *alkalinity*, as I may ſay, ſome way or other ariſing from phlogiſton, or phlogiſticated air, as *acidity* ariſes from dephlogiſticated air; and this may throw ſome light on the affinity between theſe two principles.

As to the *hepatic air* of Mr. BERGMAN, I have not given much attention to it; but it ſeems to have been proved by Mr. SENEBIER's elaborate analyſis of it, to conſiſt of the ſame elements with the liver of ſulphur from which it is made.

I ſhall now ſpeculate more largely on the ſubject of the *nitrous acid*, and the kinds of air that are procured by means of it, that the difficulties attending the ſubject may be more diſtinctly perceived, and conſequently that our attention may be excited to the ſolution of them.

As inflammable air fires readily in a phial from which it has diſplaced nitrous acid, there muſt be in this acid a *vapour* capable of combining with phlogiſton, and which may therefore be called *the dephlogiſticated vapour of ſpirit of nitre.* This vapour is abſorbed by water; for if the inflammable air be transferred through water into another veſſel, it burns only as uſual. May it not be this vapour, loaded with phlogiſton (with which it has an affinity) and ſome little nitrous acid (which, perhaps, cannot be wholly excluded, but which may be conſidered as an extraneous ſubſtance) that conſtitutes the *nitrous air*, which is produced when metals are diſſolved in this acid, and may not one part of this nitrous air, on loſing ſome of its phlogiſton, by means of the contact with iron, become what I have called dephlogiſticated nitrous air, at the ſame time that the other part, being ſuper-ſaturated with phlogiſton, becomes phlogiſticated air.

As nitrous acid is reproduced when dephlogiſticated air is admitted to nitrous air, or, according to this theory, to dephlogiſticated nitrous vapour and phlogiſton, do not theſe principles, together with *water*, which is requiſite at leaſt to the forming of an union between them, make nitrous acid. If ſo, this acid muſt conſiſt of dephlogiſticated air, dephlogiſticated nitrous vapour, phlogiſton, and water. In the ſolution of metals in this acid, then, the firſt and laſt-mentioned of theſe four elements, together with part of the third, muſt unite with the calx of the metal, while the ſecond, joined with the remainder of the third (or which is the ſame thing with phlogiſton from the metal) conſtitutes nitrous air. I do not need to add that *latent heat* ſeems to be a neceſſary ingredient in every kind of air.

On the other hand, on the ſuppoſition that the nitrous air itſelf contains all the neceſſary elements of the nitrous acid, it may be ſuppoſed that when dephlogiſticated

and

and nitrous air are mixed, the former of them, uniting with the phlogiſton of the latter, makes that *water* which is neceſſary to the liquid ſtate of the nitrous acid. But I am inclined to prefer the other hypotheſis; becauſe when the two kinds of air are mixed in a veſſel of dry mercury, no liquid is produced, and if the ſurface of the mercury be ſmall, it is a long time before the redneſs, occaſioned by the mixture, diſappears.

Iron, or the ſcales of iron, ſeem to give phlogiſton to nitrous air, which is by that means ſuper-ſaturated, and becomes phlogiſticated air; though this is a proceſs not eaſy to be accounted for. For why ſhould one part of the nitrous vapour be deprived of its phlogiſton, while the other part, contained in the ſame veſſel, and conſequently, as far as we can perceive, being in the ſame circumſtances, is more firmly united to it. It may, perhaps, therefore, be more probable, that it is the nitrous vapour in the nitrous air that joins the iron, while the phlogiſton alone remains united with the principle of heat; and that this is that which conſtitutes phlogiſticated

giſticated air, though ſome nitrous acid vapour may remain incorporated with it, which is abſolutely neceſſary to explain Mr. CAVENDISH's experiment. The quantity of nitrous acid contained in a given quantity of phlogiſticated air, ſhould be determined, in order to remove this doubt.

The hypotheſis I have mentioned is favoured by an obſervation which I have lately made, viz. that *iron*, which contains phlogiſton, decompoſes nitrous air much more readily than the *ſcales of iron*, which contain little or no phlogiſton, and eſpecially by the iron gaining weight, in conſequence of its being uſed to produce this change in nitrous air. Indeed, in this proceſs with the ſcales of iron, the nitrous air is decompoſed ſo very ſlowly, and imperfectly, that it may be ſuppoſed that it is only done by means of ſome little phlogiſton that may remain in thoſe ſcales, and the known affinity between phlogiſton and this vapour, make it more probable that this may be the caſe.

Still, however, the former difficulty is not quite removed; becauſe part of the nitrous air is left without its phlogiſton, while the other part remains more firmly combined with it. But then, in a courſe of time, all the dephlogiſticated nitrous air will unite with the iron, and all that is left in the veſſel will be phlogiſticated air. It ſeems, therefore, as if the union between the phlogiſton and the nitrous vapour muſt firſt be looſened before there can be a complete union between that vapour and the iron, and that it is in this intermediate ſtate only that the dephlogiſticated nitrous air can be obtained.

Another difficulty attending this explanation is, that in the nitrous air the nitrous vapour is already ſaturated with phlogiſton, and therefore could not be made to quit that, in order to join the ſame principle in the iron. To this I can only ſay, that the calx of the iron and the phlogiſton together may have a more powerful attraction to the nitrous vapour than its own phlogiſton, joined (as it muſt be in the form of air) with the principle of *heat*. We ſee, in other caſes, that the ſame principle in one mode of combination will have a ſtronger affinity to any particular ſubſtance than it has in another. Thus all the three mineral acids can never receive ſo much phlogiſton while they are in a liquid form, but that they will diſſolve metals, which, in the caſe of the vitriolic and nitrous acids, is always ſuppoſed to be by means of the affinity which thoſe acids have to the phlogiſton of metals.

As dephlogiſticated nitrous air is in part converted into common air, that is part of it into dephlogiſticated air, by the electric ſpark, and alſo by heating bits of crucibles in it, does it not appear, that it contains the proper element of this air, as well as it does that of nitrous air, but wants a ſufficient quantity of *latent heat*, which is imparted to it in theſe proceſſes. Perhaps, alſo, it may be found, that fixed air, the acid, and alkaline airs, or any others that are readily imbibed by water, contain leſs latent heat than thoſe kinds of air which are leſs affected by water. Very little heat is produced by the union of acid or alkaline air and water, though, as I have found by experiment, there is *ſome* produced, whereas the decompoſition of dephlogiſticated and inflammable air never fails to produce a very great degree of heat.

The phenomena attending the accenſion of pyrophorus throw ſome difficulty into the theory relating to the part that the *air* has to do in it. For by its firing equally well in dephlogiſticated and nitrous air, it ſhould ſeem that there is ſomething in common to them both, and that the dephlogiſticated nitrous air (in which it will not take fire) has not this common ingredient; which, however, the preceding theory ſuppoſes. The firing of pyrophorus probably ariſes from the ſudden abſorption of any kind of air, or water, which then gives out its latent heat (for there can be no doubt that every fluid in the form of air ſo far reſembles ſteam, as to contain latent heat). But the accenſion of it ſhould ſeem to be effected by the attraction that the pyrophorus has to ſome one principle on which the elaſtic form of the air depends; and, according to the preceding theory, there is nothing but the *dephlogiſticated nitrous vapour* that equally enters into the compoſition of dephlogiſticated and of nitrous air; and that is likewiſe found in dephlogiſticated nitrous air, in which it will not take fire. Perhaps, therefore, this accenſion may depend on the *mode of combination* between the principle of latent heat, and other principles in the conſtitution of theſe kinds of air, and of this we are entirely ignorant.

The action of *electricity* on the different kinds of air is very little known. In ſome caſes it ſeems to communicate phlogiſton, but more generally mere *latent heat.* It is however, a ſubject deſerving of the moſt particular attention, and it ſeems to be not far removed from our reach.

After theſe theoretical ſpeculations, in which I fear I have not communicated much light, though it is as much as I have been able to get, I proceed to make ſome obſervations relating to *phlogiſton*, the exiſtence of which is at preſent a great ſubject of diſcuſſion with philoſophers; ſome maintaining that there is no ſuch thing, and

others

others holding the doctrine of STAHL on the ſubject.

The term *principle* may be applied to any cauſe of a known effect, whether it be what logicians call a *ſubſtance*, or a *property*. Thus, while there is in nature ſuch a thing as *heat*, it muſt alſo be ſaid, that there exiſts a *principle*, or *cauſe of heat*, whether it conſiſt in the mere change of ſtate in the parts of the heated body (as, for inſtance, in their being thrown into a vibratory motion) or there be ſuch a *ſubſtance* (in the ſame ſenſe in which air, or water, are ſubſtances) infuſed into the body when it is heated, and withdrawn again when it is cold.

According to STAHL, phlogiſton is a real ſubſtance, capable of being transferred from one body to another; its preſence or abſence making a remarkable difference in the properties of bodies, whether it add to their *weight*, or not. Thus he concluded, that oil of vitriol, deprived of water, and united to phlogiſton, becomes ſulphur; and that the calces of metals, by the addition of the ſame ſubſtance, become metals. The *air* that has ſince been diſcovered in the calces of metals, makes no great difference in the ſyſtem. For as oil of vitriol muſt part with its water, as well as imbibe phlogiſton, in order to its becoming ſulphur; ſo the calx muſt part with its air, as well as imbibe phlogiſton, in order to become a metal.

What is now contended for is, that in the oil of vitriol changing into ſulphur, ſomething is loſt, and nothing gained, and alſo that a calx becomes a metal by the loſs of air only. And did facts correſpond to this theory, it would certainly be preferable to that of STAHL, as being more ſimple; there being one principle leſs to take into our account, in explaining the changes of bodies. But I do not know of any caſe in which phlogiſton has been ſuppoſed to enter into a body, but there is room to ſuppoſe, that ſomething does enter into it; and in other caſes, and eſpecially ſome of my own late experiments, ſomething certainly does. At leaſt, the hypotheſis that has been propoſed in its place, concerning the conſtitution of thoſe bodies, which had been ſaid to contain phlogiſton, is clearly overturned by them.

What has been inſiſted upon, as moſt favourable to the excluſion of phlogiſton, is the revival of mercury, without the addition of any other ſubſtance, from the precipitate *per ſe*. In this caſe it is evident, that mere *heat*, either in a cloſe retort, or in *vacuo*, is ſufficient to revive the metal. And as what is expelled from this calx is the pureſt dephlogiſticated air, it has been ſaid, that mercury is changed into this calx by imbibing pure air, and therefore becomes a metal again, merely in conſequence of parting with that air.

But Mr. Kirwan explains this caſe in the following manner, which to me appears perfectly ſatisfactory. The metal, when expoſed to a certain degree of heat, in contact with pure air, or with any thing that contains pure air (as in that very fine experiment of the Count de Morozzo with fixed air) imbibes, indeed, the pure air, and nothing elſe; but the phlogiſton belonging to the metal unites with that air, ſo as together to form *fixed air*; and therefore the calx may be ſaid to be the metal united to fixed air. Then, in a greater degree of heat than that in which the union was formed, this factitious fixed air is again decompoſed; the phlogiſton in it reviving the metal, while the pure air is ſet looſe. Conſequently the precipitate actually contains within itſelf all the phlogiſton that is neceſſary to the revival of the mercury*. Since, therefore, this fact *can* be accounted for without excluding phlogiſton, the ſuppoſition of which is exceedingly convenient, if not abſolutely neceſſary, to the explanation of many other facts in chemiſtry, it is at leaſt adviſeable not to abandon it.

Beſides, as it is known that *light* contains phlogiſton, if there be any ſuch thing, and there can be no *red heat* without light; it may be ſaid, that all the phlogiſton that is neceſſary to the revival of this particular metal may enter into the heated calx, notwithſtanding all our endeavours to exclude it. But it does not yet appear that light can impart ſo much phlogiſton, as ſenſibly to add to the weight of any body; and therefore it can hardly be ſuppoſed to impart ſo much as is neceſſary to the revival of a metal, the phlogiſton in which is probably half the weight of the inflammable air with which it is revived.

---

* Since it is known how much dephlogiſticated air is got from a given quantity of precipitate, and how much phlogiſton this dephlogiſticated air requires in order to compoſe fixed air, we may, from theſe *data*, eaſily aſcertain how much phlogiſton enters into the compoſition of mercury.

But the experiment which, in my opinion, proves deciſively that the principle which has hitherto been called phlogiſton is a real *ſubſtance*, and even adds conſiderably to the *weight* of bodies, is that of the decompoſition of charcoal by the heat of a burning lens in vacuo, or by means of ſteam in a hot earthen tube. According to Mr. Lavoisier, charcoal is a ſimple ſubſtance, capable of decompoſing the water that comes into contact with it, but contributing nothing to the inflammable air that is procured in the proceſs. But here we ſee almoſt the whole of the charcoal diſappear, at the ſame time that nothing but inflammable air (though not of the moſt ſimple kind) is produced. Nothing is found of the charcoal, beſides a very inconſiderable portion of *white aſhes*, which could not poſſibly have imbibed the dephlogiſticated air belonging to the water, and by that means have ſet the inflammable air at liberty; which might be ſaid with reſpect to the ſimilar experiment with iron.

It may be ſaid, that the dephlogiſticated air from the water goes to form the *fixed air* that is likewiſe produced in this proceſs, together with the inflammable air. But I have obſerved, that when no more water is uſed than what is abſolutely neceſſary for the purpoſe (as in my experiment with charcoal in vacuo) there is no fixed air mixed with the inflammable. What then becomes of the ſubſtance of the charcoal, if it does not ſupply ſomething which may be called phlogiſton from which the inflammable air is made? The air certainly exceeds the weight of the charcoal that is deſtroyed, but the ſubſtance of the charcoal being found no where elſe, muſt be in the air, as a conſtituent part of it.

It will be ſaid, that the air that is formed by heating charcoal, and combining it with ſteam, is not mere water and phlogiſton, becauſe, when it is decompoſed together with dephlogiſticated air, a conſiderable quantity of *fixed air* is produced. But as fixed air appears from the lateſt diſcoveries not to be a ſimple ſubſtance, but to conſiſt of pure air, and ſomething elſe that anſwers to the definition of phlogiſton, this principle in that fixed air muſt have been furniſhed by ſomething in the charcoal. And as almoſt the whole of the charcoal goes into the air that is produced, it muſt neceſſarily make part of its *weight*, how difficult ſoever it may be to aſcertain the exact amount of that weight.

Whatever may be objected to this reaſoning, the experiment with charcoal and the ſcales of iron, which together yield abundance of inflammable air, is, as I have obſerved, a clear refutation of Mr. LAVOISIER's hypotheſis, which was thought to overturn the doctrine of phlogiſton. For he ſuppoſes, that neither of theſe ſubſtances contain any thing that can conſtitute inflammable air.

If this hypotheſis of Mr. LAVOISIER cannot explain the preſent known facts, and the doctrine of phlogiſton *can* do it, the latter ought as yet to be retained, in preference to his, or any other that has yet been propoſed.

That calces do not become metals merely by parting with the air they contain, is evident from my experiments on heating them in contact with inflammable air, in which the inflammable air, or ſome neceſſary part of it, is undoubtedly abſorbed; and though a little moiſture be depoſited in the proceſs, it may well be ſuppoſed to be that which, in conjunction with phlogiſton, conſtituted the inflammable air. And what can the other principle that is abſorbed by the calx be, but the ſame thing, which, when united to water, is recovered again from the metal, and found to be inflammable air, having all the ſame properties with that which was employed in the revival of it. Metals, therefore, are not ſimple ſubſtances, but conſiſt of their calces, and ſomething elſe, which they take from inflammable air. And as the ſame may alſo be taken from any combuſtible ſubſtance, it correſponds exactly to STAHL's phlogiſton, and therefore the doctrine of it is confirmed by theſe experiments; that is, we muſt ſtill ſay, that, in all combuſtible ſubſtances, there is a principle capable of being transferred to other ſubſtances, which, when united to the calces of metals, makes them to be metals, and which, united to oil of vitriol (deprived of its water) makes it to be ſulphur.

SECTION XXIX.

Experiments made ſince the Sections in this Work to which they relate were printed off.

1. BEING deſirous of aſcertaining, as perfectly as I could, the nature of that *black matter* which I procured by decompoſing dephlogiſticated and inflammable air, I frequently exploded great quantities of the two kinds, and never failed to collect it. But I have not yet been able perfectly to ſatisfy myſelf with reſpect to the nature of it. From the following experiments it ſeems pretty certain, that *mercury* is neceſſary to its production, and I have never found it in veſſels in which mercury had not been contained. I did not, however, find it to be acted upon by ſpirit of nitre ſo readily as that *black powder* which is got by agitating mercury in pure water (which can be nothing but mercury ſuperphlogiſticated)

phlogiſticated) but thought that it approached nearer to the action of that acid upon *ſoot*, the blackneſs of which is not diſcharged except by frequent affuſions of that acid.

In a former volume I have obſerved, that the black powder, procured by agitating mercury in water, becomes white running mercury the moment that it is dry; but I have lately found ſome exceptions to this obſervation. Having a large quantity of this black matter in a glaſs phial, it happened to be broken by the freezing of the water with which it was mixed. I did not notice it at the time, but I afterwards found a conſiderable quantity of running mercury in the place where the bottle had ſtood, and beſides that a thick cake of dry black matter, which however appeared to be nothing but mercury, as it was completely diſſolved in ſpirit of nitre.

I muſt obſerve farther, that ſome of this black matter which I have lately procured, by agitating mercury in water, has not always become perfect running mercury by the evaporation of the water with which it was mixed, but has ſometimes left a black ſtain upon the veſſel in which it was contained. This, however, has diſappeared by the affuſion of ſpirit of nitre. I ſhall now relate ſome particulars of the proceſſes by which I procured the black matter from the decompoſition of inflammable and dephlogiſticated air.

When there was much water in the veſſel in which the exploſions were made, there was no black matter produced, though the lower part of the veſſel contained mercury; and though by repeated exploſions in theſe circumſtances the water did become turbid, and depoſit a black ſediment, it was by no means ſo much as I always got when I only juſt moiſtened the inſide of the veſſel, by filling it with mercury immediately after there had been water in it. In this laſt caſe, all the inſide of the veſſel never failed to be made exceedingly black by every exploſion; and repeating this proceſs, I was able, by pouring in water, to collect a conſiderable quantity of black ſediment. From this it ſhould ſeem, that the inſide of the veſſel contained a ſubtle inviſible *vapour of mercury*, which became black by the phlogiſton from the decompoſed inflammable air; and this black powder, as I have obſerved, is not much affected by ſpirit of nitre, but has very much the property of *ſoot*, in that reſpect, which is a circumſtance that certainly deſerves to be farther inveſtigated.

2. I have repeated with particular care the experiment of decompoſing nitrous air by the electric ſpark; from which it appears that, in this proceſs, though not in that of heating iron in it by the ſun beams, ſome *acid* is really depoſited from the air. But it is ſo little, that it may perhaps be ſuppoſed to have been *contained* in it, as an extraneous ſubſtance, not ſeparable from it by ſtanding in water. It is alſo to be obſerved that, in this proceſs, I was never able to produce any dephlogiſticated nitrous air. Whenever the experiment was ſuſpended, the air was always found to be either nitrous or phlogiſticated.

In one experiment of this kind, to which I gave particular attention, an ounce meaſure and a half of nitrous air was reduced to 0.4 of a meaſure by electric exploſions, taken in a veſſel of mercury, with a little pure water on the ſurface of it; an iron wire introduced through the mercury, and into the air, being uſed for the purpoſe of taking the exploſion at a proper diſtance. After the greateſt poſſible diminution of the air I could perceive no change of colour in the water, but after a few hours ſtanding it had a purpliſh colour, and after a day and night, it was become of a deep red, and very turbid, evidently from the ſolution of the iron. The water was acid to the taſte, but in the ſlighteſt degree imaginable; and I could perceive by the ſmell of it, that there was a little nitrous air ſtill contained in it, though I could not perceive any redneſs on expoſing the veſſel to the open air. A lighted candle being dipped into it, was immediately extinguiſhed.

There was one circumſtance which I cannot eaſily account for in this proceſs. After the diminution of the air would proceed no farther, I obſerved that, after ſome time, there was an increaſe of the quantity of it, viz. about one-tenth of an ounce meaſure; but after this the air was gradually contracted to its former dimenſions, without any farther uſe of electricity.

I repeated this experiment with the ſame quantity of nitrous air, and it was diminiſhed in about the ſame proportion as in the preceding. But examining the water before it had been diſcoloured by the ſolution of the iron, I could only perceive ſome aſtringency in the taſte of it, though it reddened a conſiderable quantity of water tinged blue with the juice of turnſole. There was alſo ſtill ſomething nitrous in the reſiduum of this air. For being mixed in equal quantities with common air, the ſtandard was about 1.8. It contained no dephlogiſticated nitrous air, as it was not ſenſibly diminiſhed by agitation in water.

As the electric ſpark cannot be ſuppoſed to communicate ſo much phlogiſton, as the heating of iron by the ſun beams, we may be better able by this proceſs to aſcertain the quantity of phlogiſton contained in phlogiſticated air; ſince the reſiduum probably contains all the phlogiſton belonging to the nitrous air from which it is made, and if we may calculate from the number in the former of theſe experiments, phlogiſticated air muſt contain about four times as much phlogiſton as an equal bulk of nitrous air; or, as appears by my former experiments, four times as much as inflammable air made from iron by oil of vitriol, or ſteam, of which one half is probably phlogiſton, and the other half water.

3. Having heated *iron* in nitrous air, I proceeded to heat in the ſame air a piece of *charcoal*, not long after it had been ſubjected to a ſtrong heat covered with ſand. The ſun not ſhining immediately after the charcoal was introduced into the veſſel of air, (through the mercury, by which it was confined) part of the air was abſorbed; but on heating the charcoal the quantity was ſoon increaſed. Having continued the proceſs as long as I thought neceſſary, I examined

the air, and found it to be about as much as the original quantity of nitrous air, but it was all phlogiſticated air, extinguiſhing a candle, and having no mixture of fixed air in it. At one time, when I dipped a lighted candle into it, I thought there was an appearance of its containing ſomething inflammable, but it was in the ſlighteſt degree imaginable. There was, however, evidently ſomething inflammable in the air expelled by water from this charcoal afterwards.

When I heated *charcoal of copper* in nitrous air, there was a ſmall addition to it of perhaps a twentieth part; and being examined, it was found to contain two-ſevenths of its bulk of fixed air, and the reſt was phlogiſticated. This change in four ounce meaſures of the air was produced by the loſs of between one-fourth and one-half of a grain of the charcoal, and the copper was evidently revived.

That fixed air ſhould be produced in this experiment, and not in the preceding, is rather extraordinary. In this circumſtance it reſembles the experiment on the heating charcoal of copper in dephlogiſticated air.

4. It is ſomething remarkable that heating the *ſcales of iron* in nitrous air, has the ſame effect as heating iron itſelf in it, but then much more *time* is requiſite to produce the ſame effect.

I heated a quantity of ſcales of iron in nitrous air till 2.7 ounce meaſures were reduced to 1.35, and then found it to be wholly phlogiſticated air, extinguiſhing a candle, and neither affecting common air, nor being affected by freſh nitrous air. During the proceſs the ſurface of the ſcales on which the focus of the lens fell were affected in a very peculiar manner, as if they were ſuperficially melted, the parts being in a ſtate reſembling that of ebullition, which continued a long time on the ſame ſpot. No moiſture was produced in the proceſs, as when theſe ſcales are heated in inflammable air.

When I put the ſcales which had been uſed in this experiment into diluted oil of vitriol, it became white like milk, and very little air was produced, even by means of heat. I got, however, a few bubbles; ſo that with good management I could juſt be ſure that it was not fixed air, that it did not affect nitrous air, nor was affected by it.

5. I treated in the ſame manner ſome iron that had been uſed to diminiſh nitrous air by the heat of the ſun. But though I diluted the oil of vitriol with only two equal parts of water, it was but weakly acted upon, and much heat was neceſſary to effect the ſolution of it. During the proceſs much *black matter* was formed, and the inflammable air produced had an offenſive ſmell. In theſe reſpects this iron ſeemed to approach to the nature of *caſt iron*; but when it was melted by a burning lens in the open air, the parts of it were not diſperſed like thoſe of caſt iron.

6. I took the electric ſpark in a quantity of *dephlogiſticated nitrous air*, till, without any perceivable change in its quantity, it was become immiſcible with water. Then, admitting diſtilled water to it, there was no

depoſit made, though I could perceive a white incruſtation quite round the inſide of the glaſs veſſel, contiguous to the mercury by which the air was confined. But admitting to it the water in my trough, which happened to have mixed with it various ſolutions of iron, there was a copious white precipitate. This ſubſtance I had got once before, and found that by expoſure to a red heat it became of a brown colour, and adhered firmly to the pieces of earthen ware on which it was heated, thereby appearing to be ochre. The whiteneſs may have been occaſioned by the mercury.

Theſe experiments on nitrous air, and the ſeveral modifications of it, eſpecially thoſe in which iron, and other ſubſtances, are heated in them, and examined afterwards, ſeem to promiſe much more light on the ſubject, and on the doctrine of air in general. But I am obliged to leave them in the preſent unfiniſhed ſtate, in which I do not chuſe to riſk any inferences from them. When I ſhall have good ſun-ſhine, and eſpecially if I ſhould ſucceed in procuring a more powerful burning lens, I ſhall eagerly reſume the experiments, and my friends, and probably the public, will ſoon be informed of the reſult of them.

To the Account of theſe Experiments I ſhall add an Illuſtration of the Note, p. 103.

THE water expended was 336 grains, and the iron gained 267 grains. Suppoſing it to have loſt phlogiſton equal to half the weight of the inflammable air, viz. 840 ounce meaſures = 25 grains (the whole weight of that air being 50 grains) the water that really entered into the iron muſt be eſtimated at 292 grains (which is 367 + 25). This deducted from 336, leaves a remainder of 34, which is not much more than 25, or half the weight of the inflammable air.

THE

APPENDIX.

NUMBER I.

A Letter from JAMES KEIR, ESQ. F.R.S. *relating to a Metallic Pyrophorus.*

DEAR SIR,

WHEN I laſt had the pleaſure of ſeeing you, I happened to mention a new kind of metallic *pyrophorus*, that had occurred to me, the accenſion of which was accompanied with a pleaſing phenomenon, namely, the inſtantaneous revival of the metal which entered into its compoſition. You expreſſed a deſire of being informed more particularly of the attending circumſtances, which I am happy to comply with, although I doubt whether the matter is worth your notice—but this I ſubmit to you.

I filled about five-ſixths of the contents of a copper cylindrical box, which had a lid fitted to it, and which was three inches in diameter, and two inches in depth, with ſaw-duſt, which I preſſed down; and I laid upon the ſaw-duſt, as much well-waſhed *plumbum corneum*, as entirely filled the box, which I then covered with its lid. I placed the box on the coals of a chamber fire, ſo that its bottom only ſhould be in contact with the fuel, and I kept it on the fire, till no more vapour ſeemed to iſſue at the joining of the lid. I then removed it from the fire, and while it was hot, I cloſed up the joining of the lid with ſealing wax, by which means the external air was excluded. After it had ſtood in the cold about ten hours, I opened the box; and the corneous lead, which was very white before the operation, was now rendered black by the vapour which had ariſen from the ſaw-duſt, and which was obliged to paſs through the lead before it could eſcape. This black metallic maſs was no ſooner expoſed to the air, than ignited ſparks appeared, which ſpread more and more, while the lead was ſeen to revive in the form of minute globules, and the part which did not revive, was changed into a yellow powder, or calx of lead. It is to be obſerved, that before I opened the box, I placed it at the ſide of the fire, in order to melt the ſealing-wax, to enable me to ſeparate the lid. It is poſſible, that this ſmall degree of heat may be neceſſary, or conducive, to the accenſion. I ought alſo to acquaint you, that the preparation of this pyrophorus, requires nicer attentions than that of any which I am acquainted with. For a ſmall exceſs of heat will revive the lead, which will ſpoil the experiment. Alſo, if any air be admitted through the joints of the veſſels employed, the kindling property will be prevented by the abſorption of the air, which in this caſe is generally too gradual to produce inflammation. The metallic ſubſtance in this ſtate of impregnation with inflammable matter, although not a pyrophorus, is an exceedingly quick tinder. For when touched, however ſlightly, by an ignited body, it will inſtantly kindle, and the fire will ſpread over the whole piece, reviving the lead wherever it goes, and exhibiting a very beautiful example of metallic reduction, not unlike the familiar experiment of reviving the lead of a wafer containing minium at the flame of a candle; but with this difference, that the fire in the wafer requires to be kept up by flame, whereas in this metallic tinder, it ſpreads, and creeps ſpontaneouſly along without flame, over the maſs.

In the preſent unſettled ſtate of chemical theory reſpecting *combuſtion*, I will not encroach on your patience, by attempting to explain the mode of action of this and other pyrophori; although it requires but little ingenuity to frame hypotheſes, with the advantages which philoſophers now frequently indulge themſelves in, of making a free uſe of various ſuppoſed principles, as *phlogiſton*, *matter of heat*, *ſorbile principle*, *humor*, *&c.* of the exiſtence of which we have unfortunately none of the proofs, that in other inſtances are thought requiſite to evince the preſence of matter. I ſhall, nevertheleſs, make one or two remarks, which perhaps may eventually tend towards a future explanation.

As in all known inſtances of pyrophori (which word I confine to ſuch ſubſtances as having been ignited and afterwards cooled, retain the power of kindling in a degree of heat much leſs than that of ignition, upon expoſure to air) one eſſential ingredient is the inflammable matter of animals, or vegetables; for no pyrophorus has yet been made with ſulphur, without the addition of other inflammable matter: and as the coal of animal and vegetable ſubſtances properly treated, without addition of other matters, has been obſerved to poſſeſs a pyrophorous quality; there ſeems to be ſome reaſon to ſuſpect, that thoſe adventitious ſubſtances which have been employed in the preparation of pyrophori, do only act as acceſſaries, by placing the animal or vegetable matter in proper circumſtances for accenſion. Thus it is an eſtabliſhed fact, that moiſture facilitates the accenſion of Homberg's pyrophorus, and I think it is probable, that the uſe of all the ingredients, excepting the flour or ſugar, in that claſs of pyrophori, is to form a liver of ſulphur, which by its powerful attraction of moiſture, produces a degree of heat which enables the pyrophorous vegetable matter to kindle. For we know that a moderate heat will often effect the accenſion of weak pyrophori, which without this help would not kindle, and Mr. Bewley has ſhewn, that if inſtead of moiſture, heat be directly applied, the effect will be produced. That moiſture is neceſſary in the ordinary heat of the atmoſphere has been proved by Mr. Scheele, who dried the air of a veſſel containing pyrophorus ſo effectually, by means of quick-lime, that the pyrophorus remained inactive. The error of Meſſrs. Savigny and Macquer, in ſuppoſing that the attraction of moiſture was cauſed by a diſengaged concentrated acid has been fully ſhewn by ſeveral chemiſts; for no ſuch acid exiſts in the pyrophorus, but on the contrary, there is a very dry liver of ſulphur, which is equally attractive of moiſture, and equally at leaſt capable of producing heat by the union. For beſides the heat produced directly in conſequence of the chemical union of liver of ſulphur with water, an additional heat ariſes from another cauſe, viz. from the change of the ſtate of the water, or its tranſition from fluidity to ſolidity, which takes place in this inſtance. For a liver of ſulphur of this kind will imbibe a conſiderable portion of water, and remain ſtill dry.

But although the attraction of moiſture may be conſidered as facilitating the operation of the pyrophorus, I cannot agree with Meſſrs. SAVIGNY and MACQUER in conſidering it as the ſole, or principal cauſe of the accenſion. For, as we know from experiment, that pyrophori will not kindle in any but pure air, or fluid containing it; and farther, that this pure air is actually abſorbed by them; we ſeem to have reaſon to believe, that the abſorption of air by the vegetable or animal inflammable matter is the immediate cauſe of accenſion; as indeed it ſeems to be of all other kinds of combuſtion. We may be warranted, I believe, in making one ſtep farther towards the explanation of this phenomenon, by referring the cauſe of the production of heat in this inſtance of abſorption of air, to the general fact or law, which ſeems to be now pretty well eſtabliſhed, namely, that heat is produced in all caſes, in which bodies undergo a change of condition, whereby they are made to occupy leſs ſpace than before, and eſpecially when they paſs from a fluid to a concrete ſtate.

In what manner the *plumbum corneum* in the pyrophorus, I have deſcribed, contributes to facilitate the accenſion of the inflammable matter of the ſaw-duſt, which impregnates the lead, I know not, unleſs by furniſhing to this very fine charcoal, or condenſed vapour of the ſaw-duſt, a proper receptacle which detains it, and gives it a very enlarged ſurface, by which the air may be very copiouſly applied. I have endeavoured to make a ſimilar pyrophorus by treating minium, and ceruſs, in the ſame manner, but by no means with equal ſucceſs. The difference may perhaps be owing to the much greater ſolidity of the laſt mentioned preparations of lead, than of the plumbum corneum. Conſequently they are leſs capable of giving an extended ſurface, on which the inflammable vapour of the ſaw-duſt might depoſite itſelf.

I am, with reſpect and eſteem,

Dear Sir,

Your moſt faithful Friend,

and moſt obedient Servant,

WINSON-GREEN,
MARCH 31, 1786.

JAMES KEIR.

NUMBER II.

A Letter from Mr. THOMAS HENRY, F.R.S. *concerning the Phlogiſton of Alkali.*

DEAR SIR, *Mancheſter, March* 22, 1786.

I RECEIVED your obliging meſſages, through the hands of ————————. I do not recollect any thing that has occurred to me, worthy of communication, except you ſhould deem the following experiment, and the inferences drawn from it, of ſufficient importance. But ſhould they appear to you unworthy of public notice, I rely on your friendſhip to ſuppreſs them.

The diſcovery of the identity of phlogiſton and inflammable gas; or, that inflammable gas is *little elſe* beſides phlogiſton, ſuggeſted to me the thought that, probably, phlogiſticated alkali might be readily prepared, by throwing ſtreams of that gas into a ſolution of cauſtic fixed alkali. I accordingly filled the middle part of a ſmall Nooth's machine, the capacity of which was about half a pint, with ſuch a ſolution; and continued, for ſome days, to paſs a current of inflammable gas, from iron filings and vitriolic acid, contained in the lower part of the machine through the liquor. The ſolution acquired a browniſh caſt, and conſiderable quantities of gas, confined between the middle and higheſt compartments of the machine, diſappeared, *though I will not ſay they were abſorbed*; notwithſtanding the cauſtic and alkaline taſte of the ſolution was not impaired, nor did it acquire the property of precipitating Pruſſian blue, from a ſolution of martial vitriol. The veſſel being left in a window, after I ſuppoſed the diſcharge of gas had been finiſhed, the ſun's rays falling on it, produced a freſh ſeparation, and ſo rarified it, that the veſſel burſt with a loud exploſion. I was, therefore prevented from examining whether the ſolution had really abſorbed any conſiderable portion of the gas.

I was at that time unacquainted with the diſcovery made by Mr. SCHEELE and others, that the matter which is united to fixed alkali, during its calcination with blood, is not pure phlogiſton, but a peculiar acid. Yet I conſidered the above experiment as affording ſtrong cauſe for ſuſpicion, that mere phlogiſton is not capable of neutraliſing, or rendering mild, alkaline ſalts; and as affording a probable anſwer to the arguments that have been uſed by Mr. DELAVAL, in his excellent paper *on the cauſe of the permanent colour*

*colour of opake bodies**, to ſhew that the power, which fixed air poſſeſſes of reducing alkaline ſalts to a mild ſtate, depends, not on any acid quality, but on the phlogiſton which forms one of its conſtituent parts. For were this opinion well founded, it might reaſonably be expected that inflammable gas, which contains ſo much larger a proportion of phlogiſton than is combined in fixed air, ſhould be, at leaſt, equally capable of deſtroying the cauſticity of alkalis.

I am, with ſincere eſteem,
Dear Sir,
Your faithful and obliged
Friend, and Servant,
THOMAS HENRY.

---

* Mancheſter Literary and Philoſophical Memoirs, vol. II.

A

CATALOGUE OF BOOKS,

WRITTEN BY

DR. PRIESTLEY,

AND PRINTED FOR

J. JOHNSON, Bookſeller, No. 72, St. Paul's Church Yard,
LONDON.

1. THE Hiſtory and Preſent State of ELECTRICITY, with original Experiments, illuſtrated with Copper-plates, 4th Edition, corrected and enlarged, 4to. 1l. 1s. Another Edition, 2 vols. 8vo. 12s.

2. Familiar Introduction to the Study of ELECTRICITY, 4th Edition, 8vo. 2s. 6d.

3. The Hiſtory and Preſent State of Diſcoveries relating to VISION, LIGHT, and COLOURS, 2 vols. 4to, illuſtrated with a great Number of Copper-plates, 1l. 11s. 6d. in boards.

4. A Familiar Introduction to the Theory and Practice of PERSPECTIVE, with Copper-plates, 2d Edition, 5s. in boards.

5. Experiments and Obſervations on different Kinds of AIR, with Copper-plates, 2d Edition, 3 vols. 18s. in boards.
N. B. In this Work are included the *Directions for impregnating Water with fixed Air*, which were formerly publiſhed ſeparately.

6. Experiments and Obſervations relating to various Branches of Natural PHILOSOPHY, with a Continuation of the Experiments on AIR, 2 vols. 12s. in boards.

7. A New CHART of HISTORY, containing a View of the principal Revolutions of Empire that have taken Place in the World; with a Book deſcribing it, containing an Epitome of Univerſal Hiſtory, 4th Edition, 10s. 6d.

8. A CHART of BIOGRAPHY, with a Book containing an Explanation of it, and a Catalogue of all the Names inſerted in it, 6th Edition, very much improved, 10s. 6d.

9. The RUDIMENTS of ENGLISH GRAMMAR, adapted to the Uſe of Schools. 1s. 6d. bound.

10. The above GRAMMAR, with Notes and Obſervations, for the Uſe of thoſe who have made ſome Proficiency in the Language. The 4th Edition, 3s. bound.

11. INSTITUTES of NATURAL and REVEALED RELIGION, in two volumes, 8vo. 2d. edition, price 10s. 6d. in boards.

12. OBSERVATIONS relating to EDUCATION: more eſpecially as it reſpects the Mind. To which is added, An Eſſay on a Courſe of liberal Education for Civil and Active Life, with Plans of Lectures on, 1. The Study of Hiſtory and General Policy. 2. The Hiſtory of England. 3. The Conſtitution and Laws of England, 4s. ſewed.

13. A COURSE of LECTURES on ORATORY and CRITICISM, 4to. 10s. 6d. in boards.

14. An Eſſay on the Firſt Principles of Government, and on the Nature of Political, Civil, and Religious LIBERTY, 2d Edition, much enlarged, 4s. ſewed. *In this Edition are introduced the* Remarks on Church Authority, in Anſwer to Dr. Balguy, *formerly publiſhed ſeparately.*

15. FORMS of PRAYER, and other Offices, for the Uſe of Unitarian Societies. Price 3s. in boards.

16. An Examination of Dr. REID's Inquiry into the Human Mind, on the Principles of Common Senſe, Dr. BEATTIE's Eſſay on the Nature and Immutability of Truth, and Dr. OSWALD's Appeal to Common Senſe, in Behalf of Religion, 2d Edit. 5s. ſewed.

17. HARTLEY's THEORY of the HUMAN MIND, on the Principle of the Aſſociation of Ideas, with Eſſays relating to the Subject of it, 8vo. 5s. ſewed.

18. DISQUISITIONS relating to MATTER and SPIRIT. To which is added, the Hiſtory of the Philoſophical Doctrine concerning the Origin of the Soul, and the Nature of Matter; with its Influence on Chriſtianity, eſpecially with reſpect to the Doctrine of the Pre-exiſtence of Chriſt. Alſo the Doctrine of Philoſophical Neceſſity illuſtrated, the 2d Edition enlarged and improved, with Remarks on thoſe who have controverted the Principles of them, 2 vols. 10s. 6d. in boards.

19. A FREE DISCUSSION of the DOCTRINES of MATERIALISM and PHILOSOPHICAL NECESSITY, in a Correſpondence between Dr. PRICE and Dr. PRIESTLEY. To which are added by Dr. PRIESTLEY, an INTRODUCTION, explaining the Nature of the Controverſy, and Letters to ſeveral Writers who have animadverted on his Diſquiſitions relating to Matter and Spirit, or his Treatiſe on Neceſſity, 8vo. 6s. ſewed.

20. A Defence of the Doctrine of NECESSITY, in two Letters to the Rev. Mr. John Palmer, 3s.

21. A Letter to JACOB BRYANT, Eſq; in Defence of Philoſophical Neceſſity, 1s.

22. The DOCTRINE of DIVINE INFLUENCE on the HUMAN MIND conſidered, in a Sermon publiſhed at the Requeſt of many Perſons who have occaſionally heard it, 1s.

The three preceding Articles may be properly bound up with the Illuſtrations of the Doctrine of Philoſophical Neceſſity.

23. An HISTORY of the CORRUPTIONS of CHRISTIANITY, with a general Concluſion, in two Parts. Part 1. Containing Conſiderations addreſſed to Unbelievers, and eſpecially to Mr. GIBBON. Part 2. Containing Conſiderations addreſſed to the Advocates for the preſent Eſtabliſhment, and eſpecially to Biſhop HURD, 2 vols. 8vo. price 12s. in boards, or 14s. bound.

24. An HISTORY of EARLY OPINIONS concerning JESUS CHRIST, compiled from Original Writers; proving that the Chriſtian Church was at firſt Unitarian, 4 vols. octavo, price 1l. 8s. ~~in boards.~~ bound

25. LETTERS to Dr. HORSLEY, Archdeacon of St. Alban's, in two Parts, containing farther EVIDENCE that the Primitive Chriſtian Church was Unitarian, price 6s.

26. A Reply to the Animadverſions on the Hiſtory of the Corruptions of Chriſtianity, in the Monthly Review for June, 1783; with Obſervations relating to the Doctrine of the Primitive Church, concerning the Perſon of Christ, 8vo. price 1s.

27. Remarks on the Monthly Review of the Letters to Dr. Horsley; in which the Rev. Mr. Samuel Badcock, the writer of that Review, is called upon to defend what he has advanced in it, price 6d.

28. Letters to a Philoſophical Unbeliever. Part 1. Containing an Examination of the principal Objections to the Doctrines of *Natural Religion*, and eſpecially thoſe contained in the Writings of Mr. Hume, 3s. ſewed.

29. Additional Letters to a Philoſophical Unbeliever, in Anſwer to Mr. William Hammon, 1s. 6d.

30. A Harmony of the Evangelists in Greek: To which are prefixed Critical Diſſertations in Engliſh, 4to. 14s. in boards.

31. A Harmony of the Evangelists in *Engliſh*; with Notes, and an occaſional Paraphraſe for the Uſe of the Unlearned; to which are prefixed, Critical Diſſertations, and a Letter to the Biſhop of Oſſory, 4to. 15s. in boards.
N. B. *Thoſe who are poſſeſſed of the* Greek Harmony, *may have this in* Engliſh *without the* Critical Diſſertations.

32. Three Letters to Dr. Newcome, Biſhop of Waterford, on the Duration of our Saviour's Miniſtry, 3s. 6d.

33. A Free Address to Protestant Dissenters, on the Subject of the Lord's Supper, 3d Edition, with Additions, 2s.——N. B. The Additions to be had alone, 1s.

34. An Address to Protestant Dissenters, on the Subject of giving the Lord's Supper to Children, 1s.

35. A Free Address to Protestant Dissenters, on the Subject of Church Discipline; with a preliminary Diſcourſe concerning the Spirit of Chriſtianity, and the Corruptions of it by falſe Notions of Religion, 2s. 6d.

36. A Sermon preached before the Congregation of Proteſtant Diſſenters at Mill Hill Chapel, Leeds, May 16, 1773, on Occaſion of the Author's reſigning his Paſtoral Office among them, 1s.

37. A SERMON preached December 31, 1780, at the New Meeting-Houſe, in Birmingham, on undertaking the Paſtoral Office in that Place, 1s.

38. The proper Conſtitution of a Chriſtian Church conſidered, in a Sermon preached at Birmingham, November 3, 1782; to which is prefixed a Diſcourſe relating to the preſent State of thoſe who are called Rational Diſſenters, price 1s.

39. TWO DISCOURSES. 1. On HABITUAL DEVOTION. 2. On the DUTY of not LIVING to OURSELVES; both preached to Aſſemblies of Proteſtant Diſſenting Miniſters, and publiſhed at their Requeſt, price 1s. 6d.

40. The Importance and Extent of FREE ENQUIRY in matters of Religion, a Sermon, preached Nov. 5, 1785; to which are added, Reflections on the preſent State of Free Inquiry in this Country, and Animadverſions on ſome Paſſages in Mr. White's Sermons at the Bampton Lectures, Mr. Howes's Diſcourſe on the Abuſe of the Talent of Diſputation in Religion, and a Pamphlet entitled Primitive Candour, price 1s. 6d.

41. A VIEW of the PRINCIPLES and CONDUCT of the PROTESTANT DISSENTERS, with Reſpect to the Civil and Eccleſiaſtical Conſtitution of England, 2d Edition, 1s. 6d.

42. LETTERS to the Author of *Remarks on ſeveral late Publications relative to the Diſſenters, in a Letter to Doctor Prieſtley*, 1s.

43. A LETTER to a LAYMAN, on the Subject of Mr. Lindſey's Propoſal for a reformed Engliſh Church, on the Plan of the late Dr. Samuel Clarke, 6d.

N. B. *The preceding* nine Pamphlet, No. 34 to 42, *may be had uniformly bound, by giving orders for* Dr. Prieſtley's larger Tracts, 2 vols. 8vo. 10s.

44. A CATECHISM for *Children and Young Perſons*, 3d Edition, 3d.

45. A SCRIPTURE CATECHISM, conſiſting of a Series of Queſtions; with References to the Scriptures, inſtead of Anſwers, 2d Edition, 3d.

46. Watts's Hiſtorical Catechiſm, with Alterations, price 6d.

47. CONSIDERATIONS for the Uſe of Young Men, and the Parents of Young Men, 2d Edition, 2d.

48. A Serious Address to Maſters of Families, with Forms of Prayer, 2d Edition, 6d.

49. A Free Addreſs to Proteſtant Diſſenters as ſuch. By a Diſſenter. A new Edition, enlarged and corrected, 1s. 6d. An Allowance is made to thoſe who buy this Pamphlet to give away.

50. An Appeal to the ſerious and candid Profeſſors of Chriſtianity, on the following ſubjects, viz. 1. The Uſe of Reaſon in Matters of Religion. 2. The Power of Man to do the Will of God. 3. Original Sin. 4. Election and Reprobation. 5. The Divinity of Chriſt; and 6. Atonement for Sin by the Death of Chriſt, a new Edition; to which is added, a Conciſe Hiſtory of thoſe Doctrines, 2d.

51. The Triumph of Truth; being an Account of the Trial of Mr. Elwall for Hereſy and Blaſphemy, at Stafford Aſſizes, before Judge Denton, 2d Edition, 2d.

52. A Familiar Illuſtration of certain Paſſages of Scripture, relating to the ſame Subjects, the 2d Edition, 4d. or 3s. 6d. per dozen.

53. A General View of the Arguments for the Unity of God, and againſt the Divinity and Pre-exiſtence of Chriſt, from Reaſon, from the Scriptures, and from Hiſtory, 2d Edition, price 2d.

54. A Free Address to thoſe who have petitioned for the Repeal of the late Act of Parliament in favour of the Roman Catholics. Price 2d. or 12s. per Hundred to give away.

N. B. The laſt Ten Tracts *may be had all bound together, by giving Orders for* Dr. Prieſtley's ſmaller Tracts, 3s. 6d. or 36s. *per dozen to thoſe who buy them to give away.*

Alſo Publiſhed under the Direction of Dr. Priestley.

THE THEOLOGICAL REPOSITORY:

Conſiſting of Original Eſſays, Hints, Queries, &c. calculated to promote Religious Knowledge, in Four Volumes, 8vo. Price 1l. 4s. in Boards. This Work is continued, the 2d Number of the 5th Volume being lately publiſhed.